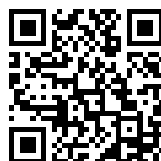


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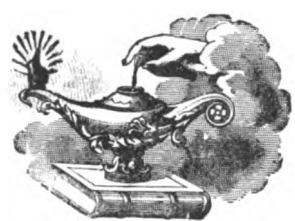


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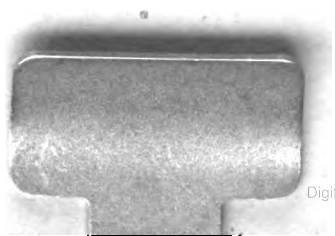
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**JOURNAL**

OF THE

**INSTITUTION OF**

**ELECTRICAL ENGINEERS,**

INCLUDING

ORIGINAL COMMUNICATIONS ON TELEGRAPHY AND  
ELECTRICAL SCIENCE.

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PUBLISHED UNDER THE SUPERVISION OF THE EDITING COMMITTEE,

AND EDITED BY

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1892.



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## Institution of Electrical Engineers.

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The Two Hundred and Fourteenth Ordinary General Meeting of the Institution was held at the Institution of Civil Engineers, 25, Great George Street, Westminster, on Thursday evening, January 15th, 1891—Dr. JOHN HOPKINSON, F.R.S., late President, in the Chair.

The minutes of the Annual General Meeting, held on December 11th, were read and confirmed.

The names of new candidates for election into the Institution were announced and ordered to be suspended.

The following transfers were announced as having been approved by the Council :—

From the class of Associates to that of Members—

John Alfred Chambers.

Joseph Rippon.

William Perren Maycock.

Sidney Sharp.

Francis Henry Nalder.

From the class of Students to that of Associates—

Robert C. Clay.

Duncan W. Johnston.

Arthur Henry Foyster.

George Herbert Thornton.

The CHAIRMAN: Gentlemen,—It is with much regret that I have to allude to the loss which we have sustained, since our last meeting, by the death of two of our members—viz., Mr. H. G. Erichsen, who was long the representative in this country of the Great Northern Telegraph Company, and who was for many years

a very active member of the Council; and Mr. William Lant Carpenter, who at the time of his death was an active member of the Council. Many of you knew him in connection with the Hanover Square School of Electrical Engineering. I am sure you will all share the great regret which the Council have felt at the death of these two gentlemen.

My last duty as President is the agreeable one of presenting the annual premiums to those to whom they have been awarded. The premium entitled "The Institution Premium," value £10, has been awarded to Mr. Esson for his paper on "Some Points in Dynamo and Motor Design," and consists of a silver chronograph, registering minutes and seconds—supplied by Messrs. W. Dent & Co.—and an ivory four-fold two-foot rule, by Stanley. The "Paris Electrical Exhibition Premium" has been awarded to Mr. W. F. Melhuish for his paper, "On Signalling across Rivers in India." The Council hope that Mr. Melhuish's success in taking this premium may encourage other Indian and Colonial members to bring forward papers, as such communications generally contain the results of experience which other members have not the means of obtaining. The premium consists of a three-inch astronomical telescope. [The premium having been formally accepted, the President continued:] The "Fahie Premium" has been awarded to Mr. K. L. Murray for his paper, "On the Electric Lighting of the Melbourne Centennial Exhibition," and will be forwarded to him in Australia.

I have now to resign this chair, and to thank you for the distinction you conferred upon me a year ago. That distinction was greatly increased by the fact that I am placed between two very eminent men. My predecessor was Sir William Thomson, in praise of whom words from me would be almost an impertinence. My successor—Mr. Crookes—is, as you all know, a very eminent chemist; but he is more: he has added vastly to our knowledge in various departments of pure physics, and has gained distinction also as an electrical engineer. In resigning my office to him, I feel I am resigning it to a gentleman who will add to its repute and distinction.

Professor W. CROOKES (the new President) having taken the

Chair, said: I will translate the applause you have so kindly accorded me into a happy augury of your leniency to any shortcomings I may disclose during my tenure of this office.

Mr. ALEXANDER SIEMENS: I should like to say a few words with regard to our latest Past-President. I think that the Institution may congratulate itself that the successor to Sir William Thomson has been one who is, even more than Sir William, a practical electrical engineer, and than whom we all, and especially the members of the Council, agree we could not have had a better President. He has thrown himself more heartily into the task, and devoted more time to the good of the Institution, than we have any right to expect a gentleman engaged in important practical business to do. The members of the Council particularly can appreciate the value of the time and trouble he has taken in furthering the interests of the Institution, and we can all congratulate ourselves upon the year which has just passed. In this respect I may only remind you that Dr. John Hopkinson was connected with the establishment of an important electrical railway during the past year, and this, with many other professional matters, has necessarily occupied a good deal of his time; and yet no President has taken more trouble or interest in the practical work of the Institution than he has. I have very great pleasure in moving—"That the hearty thanks of this Institution are due to Dr. John Hopkinson for the admirable manner in which he has discharged the duties of President during the past year."

Mr. GISEBERT KAPP: I should like to be allowed to second this motion, which I feel sure expresses very correctly the feeling of every member of the Institution. We are losing one good President, and are gaining another good President. But when I say we are losing the former, I hope I am wrong, because Dr. Hopkinson has taken so much and such active interest in the work of the Institution during the past year that we all of us hope he will continue to give us his most valuable assistance at the Council table and at the meetings in future.

The motion was put from the Chair, and carried unanimously.  
Dr. HOPKINSON: Gentlemen,—When I accepted the office of

President, I felt that I was accepting a duty—coupled, no doubt, with very great distinction—but a duty which would hardly be a pleasure. Well, looking back at the past year, I feel it has been a very pleasurable one indeed. I think the pleasure resulted, not so much from the nature of the duties to be performed, nor the distinction to be gained by their performance, but from the very kind feeling it has called forth on the part of those with whom I have had to act. You, gentlemen, have given me credit—or, at all events, have acted as though you have given me credit—for doing the best I could for the Institution, and have always cordially supported me in my endeavours; my colleagues on the Council have throughout given me most active and generous help; and last, but not least, our invaluable Secretary has had everything ready for me on every occasion, and has saved me every possible trouble he could, at the same time consulting me when necessary, and keeping me well informed of all that concerned the welfare of the Institution. I thank you heartily and can only say that I look back at the past year with a very great deal more pleasure than I looked forward to it in the beginning.

The PRESIDENT then delivered his Inaugural Address.

## ELECTRICITY IN TRANSITU: FROM PLENUM TO VACUUM.

### INTRODUCTION.

Whilst steadily bearing in mind that I have the honour to address a Society, not only of physicists, but of electrical engineers, I shall not, I hope, be out of order in venturing to call your attention to a purely abstract phase of electrical science. Numberless instances show that pure research is the abundant source from which spring endless streams of practical applications. We all know how speculative inquiry into the influence of electricity on the nervous systems of animals led to knowledge of current electricity, and ultimately to the priceless possession of the telegraph and the telephone. The abstract study of certain micro-

scopic forms of parasitic vegetable life has enabled us to give to fermented solutions of sugar the exact flavour and aroma of the most highly prized wines, and probably, ere long, will put us in a position to increase at will the fertility of the soil. In a different direction, the same class of abstract researches applied to medical science has brought us within measurable distance of the final conquest over a large class of diseases hitherto incurable; and without egotism I may perhaps be allowed to say that my own researches into high vacua to some extent have contributed to the present degree of perfection of the incandescence lamp. Surely, therefore, whilst eagerly reaping and storing the harvest of practical benefits, we must not neglect to scatter more seed for future results, perchance not less wonderful and valuable.

In another respect I deviate to some extent from the course taken by many of my predecessors. I am about to treat electricity, not so much as an end in itself, but rather as a tool, by whose judicious use we may gain some addition to our scanty knowledge of the atoms and molecules of matter, and of the forms of energy which by their mutual reactions constitute the universe as it is manifest to our five senses.

I will endeavour to explain what I mean by characterising electricity as a tool. When working as a chemist in the laboratory, I find the induction spark often of great service in discriminating one element from another, also in indicating the presence of hitherto unknown elements in other bodies in quantity far too minute to be recognisable by any other means. In this way, chemists have discovered thallium, gallium, germanium, and numerous other elements. On the other hand, when examining electrical reactions in high vacua, various rare chemical elements become in turn tests for recognising the intensity and character of electric energy. Electricity, positive and negative, effect respectively different movements and luminosities. Hence the behaviour of the substances upon which electricity acts may indicate with which of these two kinds we have to deal. In other physical researches both electricity and chemistry come into play simply as means of exploration.

In submitting to you certain researches in which electricity is

used as a tool, or as a means of bringing within scope of our senses phenomena that otherwise would be unrevealed, I must for a moment recall to your minds the now generally accepted theory of the constitution of matter.

#### KINETIC THEORY OF GASES.

Matter, at its ultimate degree of extension, is conjectured to be not continuous, but granular. Maxwell illustrates this view as follows:—To a railway contractor driving a tunnel through a gravel-hill, the gravel may be viewed as a continuous substance. To a worm wriggling through gravel, it makes all the difference whether the creature pushes against a piece of gravel or directs its course between the interstices. To the worm, therefore, gravel seems by no means homogeneous and continuous.

With speculations as to the constitution of liquid and solid matter I need not trouble you, but will proceed at once to the third, or gaseous, state of matter.

The kinetic theory of gases teaches that the constituent molecules dart in every possible direction with great but continually varying velocities, coming almost ceaselessly in mutual collision with each other. The distance each molecule traverses without hitting another molecule is known as its *free path*; the average distance traversed without collision by the whole number of molecules of a gas at any given pressure and temperature is called the *mean free path*. The molecules exert pressure in all directions, and are only restrained by gravitation from dissipating themselves into space. In ordinary gases, the length of the mean free path of the molecules is exceedingly small compared with the dimensions of the vessel, and the properties we then observe are such as constitute the ordinary gaseous state of matter, which depend upon constant collisions. But if we greatly reduce the number of molecules in a given extent of space, the free path of the molecules under electric impulse is so long that the number of their mutual collisions in any given time in comparison with the number of times they fail to collide may be disregarded. Hence, the average molecule can carry out its own motions without interference. When the mean free path becomes

comparable to the dimensions of the containing vessel, the attributes which constitute gaseity shrink to a minimum, the matter attains the ultra-gaseous, or "radiant," state, and we arrive at a condition where molecular motions under electrical impulse can easily be studied.

The mean free path of the molecules of a gas increases so rapidly with progressive exhaustion that whilst that of the molecules of air at the ordinary pressure is only 1-10,000th of a millimetre, at an exhaustion of a hundred-millionth of an atmosphere—a point (which, with present appliances, is easy to attain) corresponding to the rarefaction of the air 90 miles above the earth's surface—the mean free path will be about 30 feet; whilst at 200 miles above the earth it will be 10,000,000 miles, and millions of miles out in the depths of space it will become practically infinite. I could go on speculating, in spite of Aristotle, who said: "Beyond the universe there are neither "space nor vacuum, nor time."

In discussing the motions of molecules we have to distinguish the *free* path from the *mean free path*. Nothing is yet known of the *absolute* length of the free path, nor of the *absolute* velocity, of a molecule. For anything we can prove to the contrary, these values may vary almost from zero to infinity. We can deal only with the *mean* free path and the *mean* velocity.

#### THE VACUUM PUMP.

As most of the experiments I put before you to-night are connected with high vacua, it is not out of place to refer to the pump by means of which these tubes are exhausted. Much has been said lately in recommendation of the Geissler pump and its many improvements, but I am still strongly in favour of the Sprengel, as with it I have obtained greater exhaustion than with any other. I should like to point out that the action does not stop when we cease to see air specks passing down the tubes, but continues long after this point has been passed. Neither is the non-conducting vacuum, so easily obtained by the Sprengel pump, due in any way to the presence of mercury vapour, since non-conduction can be obtained just as rapidly when special

precautions have been taken to keep mercury vapour out of the tubes.

One of the great advantages of the Sprengel pump over all others lies in the fact that its internal capacity need not exceed a few cubic centimetres, and there is, therefore, much less wall surface for gases to condense upon. I have brought the very latest modification of this form of pump here to-night, and you will have an opportunity of seeing it in action, and of measuring with the McLeod gauge the rarefaction it produces.\*

### THE PASSAGE OF ELECTRICITY THROUGH RAREFIED GAS

The various phenomena presented when an induction spark is made to pass through a gas at different degrees of exhaustion

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\* My measurements of high vacua have all been taken with the beautiful little gauge devised by Professor McLeod. Unmerited discredit has recently been cast on this gauge, the principal fault alleged being its inability to distinguish between the tension of the permanent gas and that of the mercury vapour present. Now it is evident that, under ordinary circumstances, the tension of mercury vapour may be disregarded, as it will be the same on both sides of the gauge; and it will be only in cases where no mercury is present on one side of the gauge that a slight error is introduced. It is, however, very difficult to devise and successfully experiment with apparatus in which a trace of mercury vapour shall not enter, and it is not likely that an experimentalist who would be working with such mercury-free apparatus would attempt to use the gauge without remembering that in this special case the indications would be incorrect. To use the McLeod gauge requires much patience and some amount of experience, but I have always found it trustworthy to register exhaustions far beyond the millionth of an atmosphere. I can adduce circumstantial evidence of the accuracy of its readings at these high vacua. In the year 1881 I read a paper before the Royal Society on "The Viscosity of Gases at High Exhaustions" (*Phil. Trans.*, 1881, p. 387), and illustrated my results in three large diagrams, on which I plotted the experimental results obtained at rarefactions up to the 0.02 millionth of an atmosphere, giving curves comparing the decrease in viscosity with that of the repulsion resulting from radiation, at the different pressures. Now these curves, in the case of air, for instance, are perfectly regular and uniform in their falling off, and it is evident that this could not have been the case unless the abscissæ representing viscosity and the ordinates representing pressure were equally accurate. I am satisfied that, within narrow limits, the abscissæ of viscosity are correct to the highest point, and the conformity of experiment to theory in the shape of these curves is a conclusive proof that, at as high an exhaustion as 0.02 M., the McLeod gauge is to be trusted to give accurate results within 2 per cent. of the truth. To give some idea what these high exhaustions mean, I may mention that the highest measured exhaustion—0.02 M.—bears the same proportion to the ordinary pressure of the atmosphere that a millimetre does to 30 miles, or, in point of time, that one second bears to 20 months.

point to a modified condition of the matter at the highest exhaustions. Here are three exactly similar bulbs, the electrodes being aluminium balls, and the internal pressures being respectively 75 mm., 2 mm., and 0.1 mm. If I pass the induction current in succession through the bulbs, you will perceive in each case very different luminous phenomena. Here is a slightly exhausted tube (Fig. 1), like the first in the series just exhibited

$$P = 75 \text{ mm.}$$

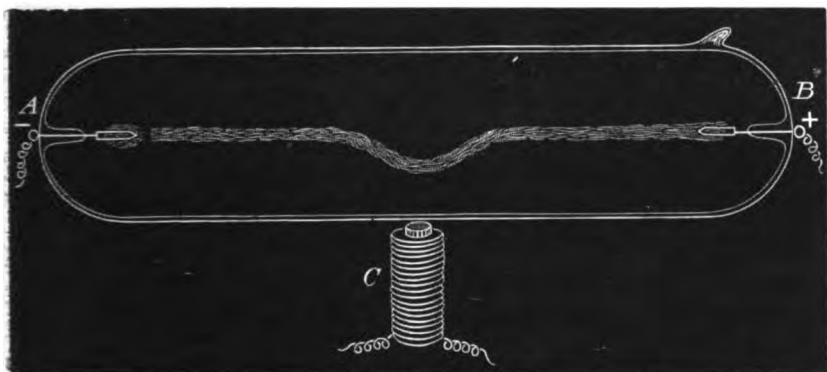


FIG. 1.

(75 mm.); the induction spark passes from one end to the other, A, B, and the luminous discharge is seen as a line of light, acting as a flexible conductor. Under the tube I have an electro-magnet, C, and on making contact the line of light dips in the centre down to the poles of the magnet, and then, rising again, proceeds in a straight line. On reversing the current the line of light curves upwards. Notice that the action of the magnet in this case is only local.

In a highly exhausted tube the action is quite otherwise. Such a tube is before you (Fig. 2), and in it I have carried the exhaustion to a high point (0.1 mm.). I pass the induction current and you perceive the electrified molecules, like the line of light in the first tube, also move in straight lines, and make their path apparent by impinging on a phosphorescent screen, D E. If, however, I submit them to the action of a magnet, C, their behaviour is different. The line dips down to F, but does

*not* recover itself. It seems that in the tube first shown we have to do with the average behaviour of the molecules of gas in its

$$\begin{aligned} P &= 0.1 \text{ mm.} \\ &= 131.5 \text{ M.} \end{aligned}$$

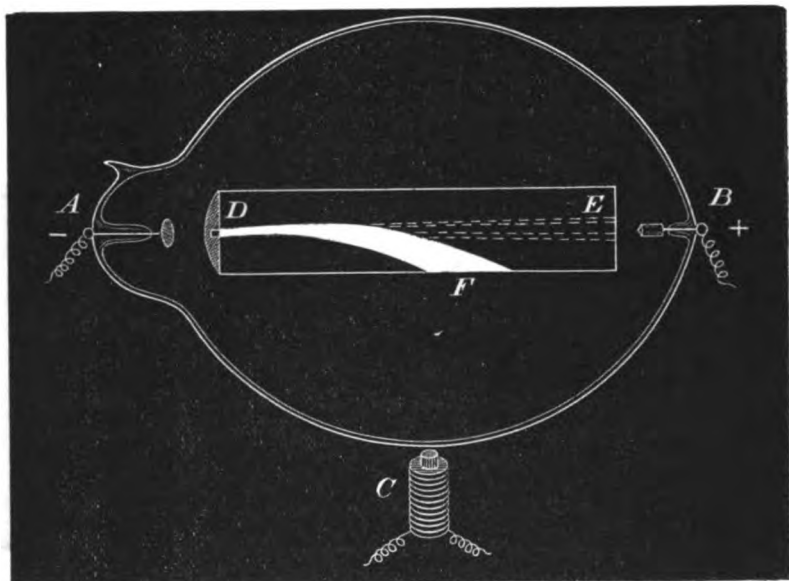


FIG. 2.

totality. In the second case, where the gas has been greatly attenuated, we are merely concerned with the behaviour of the individual molecules of which it was originally composed.

#### THE STRATIFIED DISCHARGE.

When the gas is rarer than is necessary to give the flexible line of light, as shown in the first experiment, the luminosity is plainly discontinuous, or, as it is termed, stratified.

A very good illustration of this fact may be taken from the moving crowd in any much-frequented street—say Fleet Street. If at some time when the stream of traffic runs almost equally in both directions, we take our stand at a window from which we can overlook the passing crowd, we shall notice that the throng on the foot-way is not uniformly distributed, but is made up of knots—we might almost say blocks—interrupted by spaces.

which are comparatively open. We may easily conceive in what manner these knots or groups are formed. Some few persons walking rather more slowly than the average rate slightly retard the movements of others, whether travelling in the same or in an opposite direction. Thus a temporary obstruction is created. The passengers behind catch up to the block and increase it, and those in front, passing on unchecked at their former rate, leave a comparatively vacant space. If a crowd is moving all in the same direction, the formation of these groups becomes more distinct. With vehicles in crowded streets, the result, as everyone may have remarked, will be the same.

Hence mere differences in speed suffice to resolve a multitude of passengers into alternating gaps and knots.

Instead of observing moving men and women, suppose we experiment on little particles of some substance, such as sand, approximately equal in size. If we mix the particles with water in a horizontal tube and set them in rhythmical agitation, we shall see very similar results, the powder sorting itself with regularity into alternate heaps and blank spaces.

If we pass to yet more minute substances, we observe the behaviour of the molecules of a rarefied gas when submitted to an induction current. The molecules here are free, of course, from any caprice, and simply follow the law I seek to illustrate, and though originally in a state of rampant disorder, yet under the influence of the electric rhythm they arrange themselves into well-defined groups or stratifications; the luminosities show where arrested motion, with concomitant friction, occurs, and the dark intervals indicate where the molecules travel with comparatively few collisions.

#### PARTI-COLOURED STRATIFICATIONS.

As another illustration of stratifications in a moderately exhausted tube ( $P = 2$  mm.), I will take the case of hydrogen prepared from zinc and sulphuric acid after being passed through various purifying agents, dried in the usual manner, and exhausted with a mercury pump (Fig. 3). I pass the induction current, and we see that the stratifications are tri-coloured—blue,

pink, and grey. Next the negative pole, A, is a luminous layer; then comes a dark interval, or Faraday's dark space (see below);

$$P = 2.0 \text{ mm.} \\ = 2,630.0 \text{ M.}$$

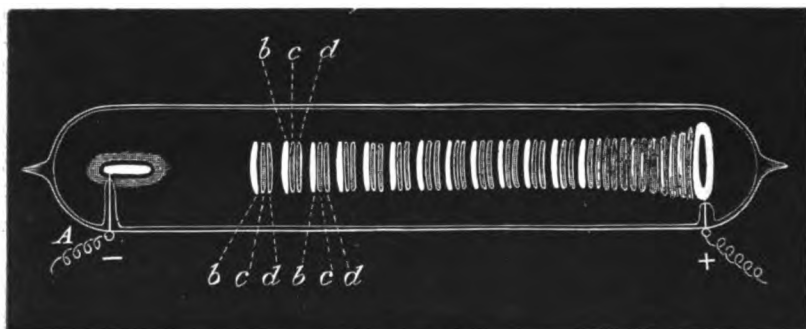


FIG. 3.

and after this are the stratifications, the front component (*b*) of each group blue, the next (*c*) pink, and the third (*d*) grey. The blue disks are somewhat erratic. At a certain stage of exhaustion all the blue components of the stratification suddenly migrate to the front, forming one bright blue disk, and leaving the pink and grey components by themselves. The tube before you (Fig. 4) is at this particular stage of exhaustion, and on

$$P = 2.0 \text{ mm.} \\ = 2,630.0 \text{ M.}$$

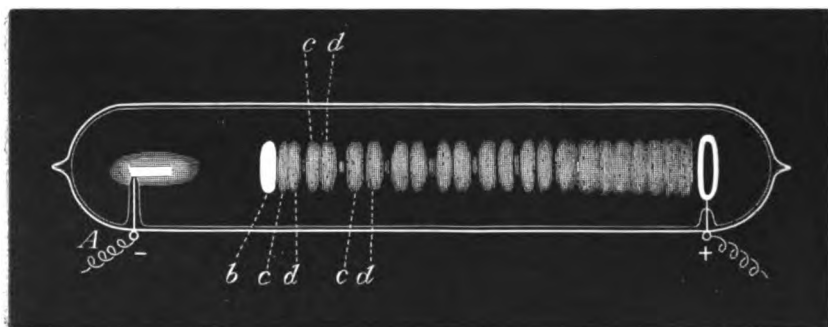


FIG. 4.

passing the current you observe the blue disk only (*b*) is in front. When the tube contains a compound gaseous residue of this kind,

the form of stratifications can be very considerably altered by varying the potential of the discharge. This alteration in the forms of stratification was first pointed out by Gassiot (1865, "B.A. Abstracts," p. 15), who gave very full descriptions and drawings of the alterations produced by putting in resistances of various lengths of distilled water. That the alteration depends simply upon the difference of potential, the following experiment pretty clearly shows:—Here is a tube giving on my coil the coloured stratification usually attributed to the presence of residual hydrogen, but which, I find, is due to a mixture of hydrogen, mercury, and hydrocarbon vapours. Now, by altering the brake so as to produce frequent discharges of lower potential, you see the stratifications gradually change in shape and become all pink; again altering the brake, so as to send less rapid discharges at a much higher potential, once more we get the coloured stratifications. When in this state, if we introduce a water resistance into the circuit, so as to damp down the potential, exactly the same thing happens. The blue disk is caused by mercury; its spectrum is that of mercury only, without even a trace of the bright red line of hydrogen. Experiments not yet finished make it very probable that the pink disks are due to hydrogen, and that the grey disks indicate carbon. The tube you have just seen contains nothing but hydrogen, mercury, and a minute trace of carbon; but with all the resources at my command I have not been able to get hydrogen quite free from impurity. Indeed, I do not think absolutely pure hydrogen has ever yet been obtained in a vacuum tube. I have so far succeeded as to completely eliminate the mercury, and almost completely to remove the trace of carbon. On the table is such a tube giving uniformly pink stratifications, and showing no blue or grey disks with any potential of current.

#### THE DARK SPACE.

After the stratification stage is passed we come to a very curious phenomenon—the so-called "dark space." Studying electrical phenomena in gases, in the year 1838, Faraday\* pointed

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\* "Experimental Researches in Electricity," 1838, par. 1544.

out a break in the continuity of the luminous discharge separating the glow of the positive electrode from that of the negative. This he called "the dark space." It is seen in tubes containing gas only slightly rarefied, as in this tube (Fig. 5— $P = 6$  mm.),

$P = 6.0$  mm.

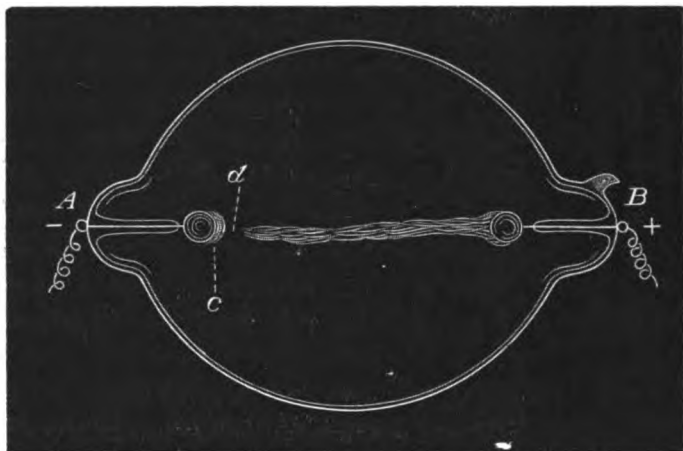


FIG. 5.

where you will observe that the positive glow, extending as a pink streak from the positive electrode, B, ends about 10 millimetres before the spot of blue light, C, representing the negative glow. This gap, or non-luminous hiatus, D, is Faraday's "dark space." Separating the negative glow from its electrode is another space. In this tube it is so small that the glow appears to be in actual contact with the electrode, but on exhausting a little further it rapidly separates; and in the next tube (Fig. 6), containing air at a little less pressure ( $P = 3$  mm.), this dark space, E, has extended so as to remove the negative glow about 4 millimetres from the electrode A. It is with this second dark space that I particularly wish to deal to-night. Therefore I shall refer to it as the "dark space," meaning always that in the negative glow.

In the experiment just shown with hydrogen stratifications the contents of the tube under the electric discharge still obey the laws following from the average properties of an immense number of molecules moving in every direction with velocities of

all conceivable magnitudes. But if I continue exhausting, the dark space, E, round the negative pole, A, becomes visible,

$$P = 3.0 \text{ mm.}$$

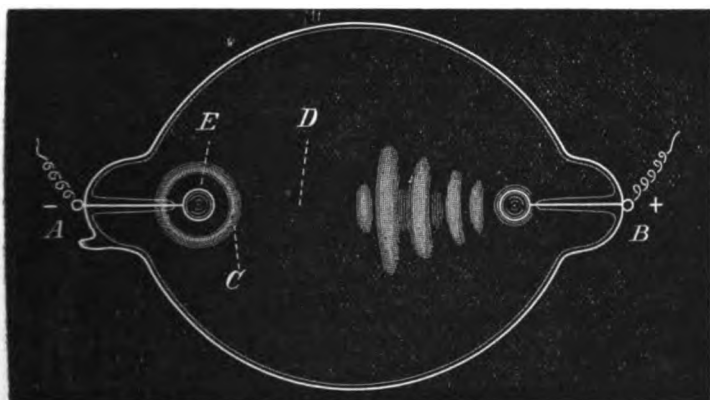


FIG. 6.

grows larger and larger, and at last fills up the entire tube. The molecules at this stage are in a condition different from those in a less highly exhausted tube. At low exhaustions they behave as gas in the ordinary sense of the term, but at these high exhaustions, under electric stress, they have become exalted to an *ultra-gaseous* state, in which very decided properties, hitherto masked, come into play.

The radius of the dark space varies with the degree of exhaustion, with the kind of gas in which it has been produced, with the temperature of the negative pole, and, to a less extent, with the intensity of the spark.

It has been erroneously assumed that I ever said the thickness of the "dark space" represents the mean free path of the molecules in their ordinary condition, and it has been pointed out that the radius of the dark space is decidedly greater than the calculated mean free path of the molecules. I have taken accurate measurements of the radius of the dark space at different pressures, and compared it with the calculated mean free path of the gaseous molecules at corresponding pressures when not under the influence of electrical energy, and I find that they do not bear a

constant relation one to the other. The length of the dark space is not 20 times the mean free path, as some have estimated, but a gradually increasing multiple must be taken as the exhaustion becomes greater.

### EXPLORATION WITH IDLE POLES.

Wishing to learn something of the electrical condition of the matter within and without the dark space, I made a tube (Fig. 7)

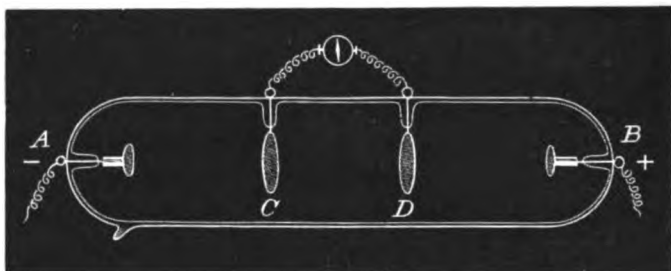


FIG. 7.

having, besides the positive and negative terminals, A, B, two extra intermediate poles, C and D. The tube showed that when the exhaustion was such that both the idle poles were outside the dark space, on passing the current through the tube, there was a considerable difference of potential between them when measured on the galvanometer. If the exhaustion was carried so high that one of the extra poles was just on the border of the dark space, then no current passed between them. When the exhaustion was still further increased, so as to inclose one of the extra poles fully in the dark space, again there was a great difference of potential between them, but the direction was reversed, the pole at highest potential now being the one formerly at lowest potential.

When the dark space has been further explored by means of a movable negative pole, I found that the effects did not depend essentially on the exhaustion, and were really due to the position occupied by the extra poles with regard to the dark space.

These phenomena are difficult to understand from mere description, and the experiments themselves are not easy to carry out so as to be visible to many at a time. I have here, however, a

working model of an apparatus which will make these puzzling indications clear to all.

$$P = 0.25 \text{ mm.} \\ = 390.0 \text{ M.}$$

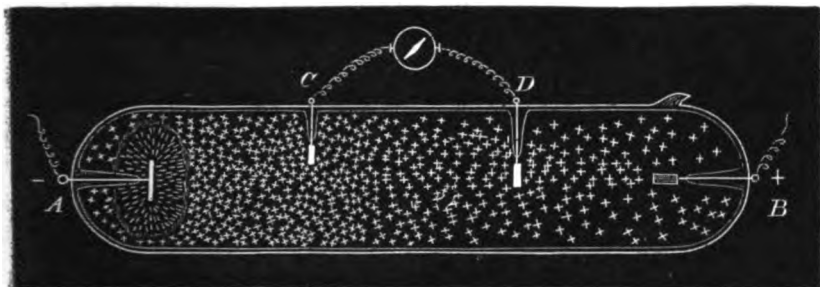


FIG. 8, a.

A cylindrical tube (Fig. 8, a, b, and c— $P = 0.25$  mm.), furnished with the usual poles, A, B, at the ends, has two extra, or idle, poles near together at C and D. The pole A is movable along the axis of the tube, so that when exhausted the dark space can be brought to any desired position with respect to the idle poles C and D. The shading and + and - marks roughly show the distribution of positive and negative electricity inside the tube. I start with the negative pole, A, as far as possible from either idle pole (Fig. 8, a). Turning on the coil, you see the dark space surrounding the pole A, and the idle poles quite outside.

$$P = 0.25 \text{ mm.} \\ = 350.0 \text{ M.}$$

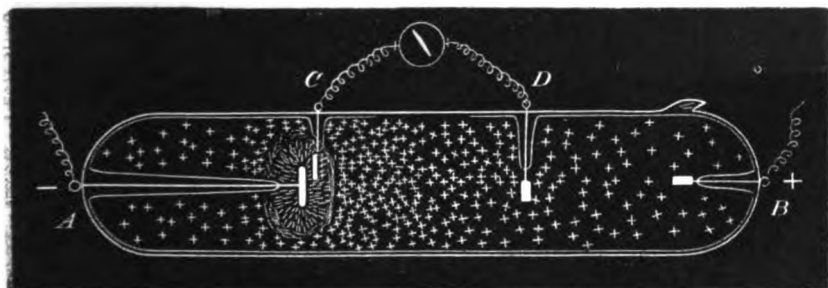


FIG. 8, b.

The shading shows that each idle pole is in the positive area, and on testing with the gold-leaf electroscope it will be seen that each is charged with positive electricity. But the shading

also shows more positive at C than at D, and on connecting C and D with a galvanometer the needle indicates a rush of current from C to D, D being negative to C.

The dark space is next brought to such a position that the pole C is well within it (Fig. 8, b). A change has now come over the indications. The galvanometer shows a reverse current to that which was seen on the former occasion. C is now negative, and D positive, but the gold leaves still tell us both poles are positively electrified.

At a certain position of the dark space, when its edge is on the pole C (Fig. 8, c), a neutral state is found at which the gold

$$P = 0.25 \text{ mm.} \\ = 380.0 \text{ M.}$$

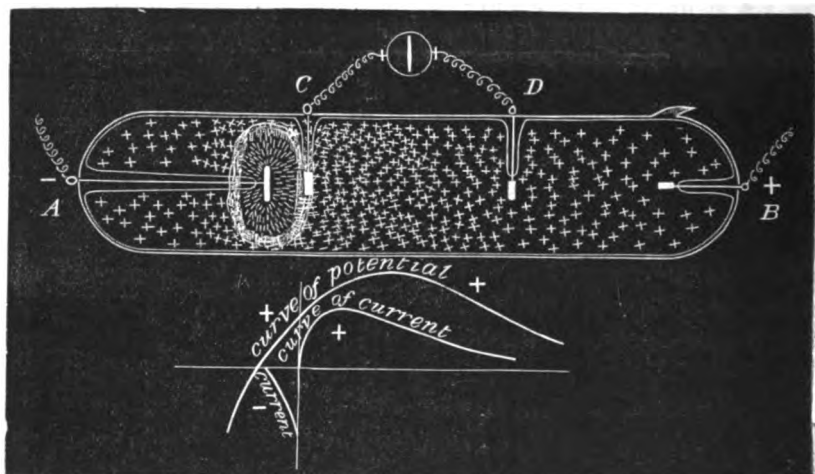


FIG 8c.

leaves still show strongly positive electrifications, and no current is seen on the galvanometer. The curves below (Fig. 8, c) roughly show the rise and fall of negative and positive current at different parts of the tube, whilst the potential curve keeps positive.

When a substance that will phosphoresce under electrical excitement is introduced into the tube, the position of greatest luminosity is found to be at the border of the dark space, just where the two opposing armies of negative and positive atoms

meet in battle array and re-combine. Later on I shall refer to this phenomenon in connection with the phosphorescence of yttria.

### RADIANT MATTER.

By means of this tube (Fig. 9) I am able to show that a

$$\begin{aligned} P &= 0.1 \text{ mm.} \\ &= 131.5 \text{ M.} \end{aligned}$$

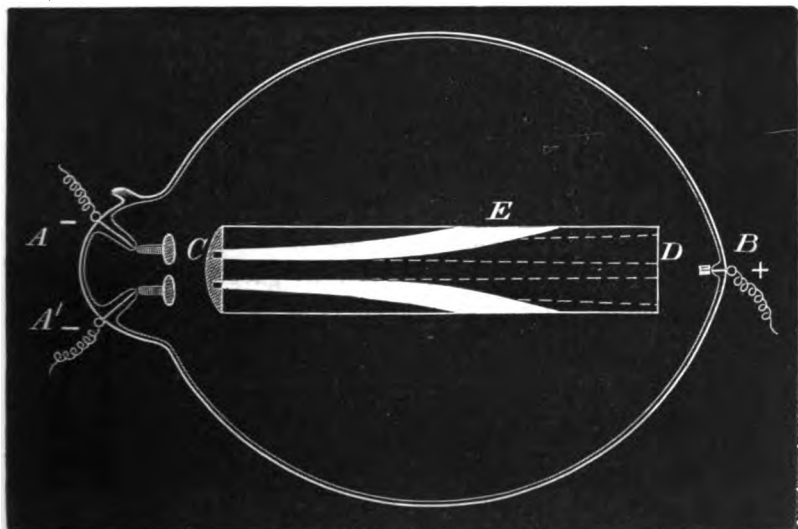


FIG. 9.

stream of ultra-gaseous particles, or radiant matter, does not carry a current of electricity, but consists of a succession of negatively electrified molecules whose electrostatic repulsion overbalances their electro-magnetic attraction, probably because their speed along the tube is less than the velocity of light. The tube has two negative terminals, A, A', close together at one end, enabling me to send along the tube two parallel streams of radiant matter, rendered visible by impinging them through holes in a mica diaphragm on a screen of phosphorescent substance. It is exhausted to a pressure of 0.1 mm. I connect one of the negative poles, A, with the induction coil, and the luminous stream darts along the tube from C to D parallel with the axis. I now connect the other negative pole, giving a second parallel stream of radiant matter. If these streams are in the nature of

wires carrying a current they will attract each other, but if they are simply two streams of electrified molecules they will repel each other. As soon as the second stream is started you see the first stream jump away in the direction C E, showing strong repulsion, proving that they do not act like current-carriers, but merely like similarly electrified bodies. It is, however, probable that were the velocity of the streams of molecules greater than that of light, they would behave differently, and attract each other, like conductors carrying a current.

To ascertain the electrical state of the residual molecules in a highly exhausted tube such as you have just seen, I introduced an idle pole, or exploring electrode, between the positive and negative electrodes in such a manner that the molecular stream might play upon it. The intention was to ascertain whether the molecules on collision with an obstacle gave off any of their electrical charge. In this experiment (Fig. 10— $P = 0.0001$  mm.,

$$\begin{aligned} P &= 0.0001 \text{ mm.} \\ &= 0.13 \text{ M.} \end{aligned}$$

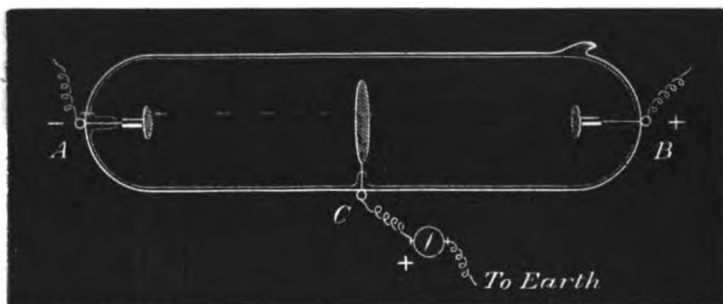


FIG. 10.

or 0.13 M.\*), it was found that an idle pole, C, placed in the direct line between the positive and the negative poles, A, B, receiving in consequence the full impact of the molecules shot from the negative pole, manifested a strong *positive* charge. In a variety of other experiments made to decide this question, the electricity obtained was always found positive on testing with the

---

\* M. = one-millionth of an atmosphere.  
 1,000,000 M. = 760 mm.  
 „ = one atmosphere.

gold leaf or Lippman's electrometer; and when the idle pole was connected to earth through a galvanometer, a current passed, as if this pole were the copper element of a copper-zinc cell, indicating leakage of a current to earth, the idle pole being positive. If, instead of sending this current to earth, the wire was connected to the negative pole of the tube, a much more powerful current passed in the same direction.

### THE EDISON EFFECT.

An exactly parallel experiment has been made by Mr. Edison, Mr. Preece, F.R.S., and Professor Fleming, using, instead of a vacuum tube, an incandescent lamp. They found that from an idle pole placed between the ends of the filament the electricity always flowed as if the pole were the zinc element of a copper-zinc cell. Having repeated their experiments, I entirely corroborate them. I got a powerful current in one direction from an idle pole placed between the limbs of an incandescent carbon filament, and one in the opposite direction from an idle pole in a highly exhausted vacuum tube. This discrepancy was extremely puzzling, and I tested, with a similar result, very many experimental tubes made in different ways. The electricity obtained from an idle pole placed between the positive and negative terminals in a highly exhausted tube was always strongly *positive*, and it is only recently that continued experiment has cleared the matter up.

Some of the contradictory results are due to the exhaustion not being identical in all cases. In my vacuum tubes the direction of current between the idle pole and the earth changes from negative to positive as the exhaustion rises higher. Testing the current when exhaustion is proceeding, there is a point reached when the galvanometer deflection—hitherto negative—becomes *nil*, showing that the potential at this point is zero. At this stage the passage of a few more drops of mercury down the pump tube renders the current positive. This change occurs at a pressure of about 2 mm.

After this point is reached, when the induction current is passed through the tube, the walls rapidly become positively electrified, probably by the friction of the molecular stream against

the glass, and this electrification extends over the surface of any object placed inside the tube. I will show you how this electrification of the inner walls of the tube acts on the molecular stream at high vacua. In this tube (Fig. 11— $P = 0.001$  mm., or

$$P = 0.001 \text{ mm.}$$

$$= 1.3 \text{ M.}$$

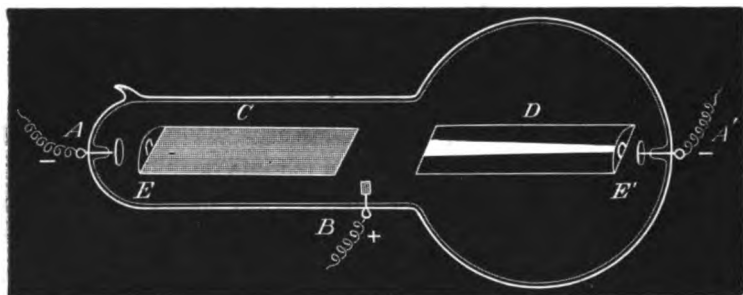


FIG. 11.

1.3 M.) are fixed two exactly similar phosphorescent screens, C and D; at one end of each is a mica gate, E, E', with a negative pole, A, A', facing it. One of the screens, C, is in the cylindrical part of the tube, and close to the walls; the other, D, is in the spherical portion, and therefore far removed from the walls. On passing

$$P = 0.0001 \text{ mm.}$$

$$= 0.13 \text{ M.}$$

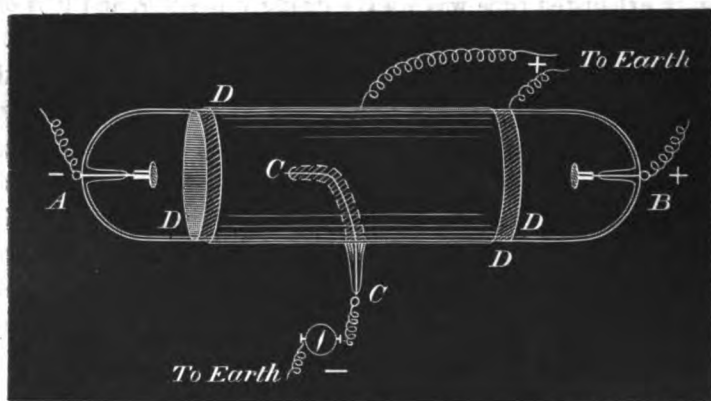


FIG. 12.

the current, the screen D in the globe shows a narrow sharp streak of phosphorescence, proving that here the molecules are free to

follow their normal course straight from the negative pole. In the cylindrical part of the tube, however, so great is the attraction of the walls that the molecular stream is widened out sufficiently to make the whole surface of the screen C glow with phosphorescent light.

If an idle pole, C C (Fig. 12— $P = 0.0001$  mm., or 0.13 M.), protected all but the point by a thick coating of glass, is brought into the centre of the molecular stream in front of the negative pole A, and the whole of the inside and outside of the tube walls are coated with metal, D D, and "earthed," so as to carry away the positive electricity as rapidly as possible, then it is seen that the molecules leaving the negative pole and striking upon the idle pole C on their journey along the tube carry a negative charge, and communicate negative electricity to the idle pole.

This tube is of interest, since it is the one in which I was first able to perceive how in my earlier results I always obtained a positive charge from an idle pole placed in the direct stream from the negative pole. Having got so far, it was easy to devise a form of apparatus that completely verified the theory, and at the same time threw considerably more light upon the subject. Fig. 13, *a*, *b*, *c*, is such a tube, and in this model I have

$$\begin{aligned} P &= 0.0001 \text{ mm.} \\ &= 0.13 \text{ M.} \end{aligned}$$

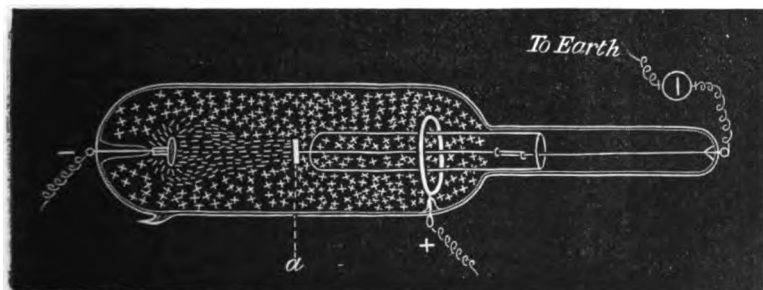
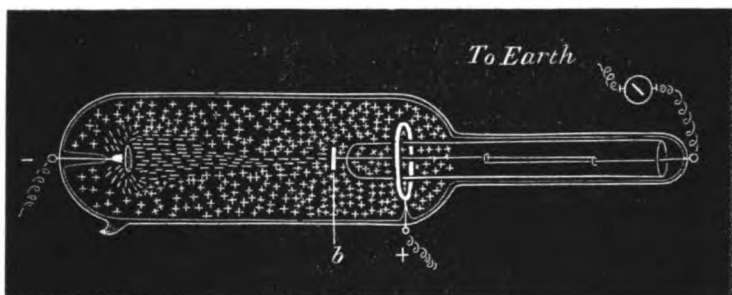


FIG. 13 a.

endeavoured to show the electrical state of it at a high vacuum by marking a number of + and - signs. The exhaustion has been carried to 0.0001 mm., or 0.13 M., and you see that in the neighbourhood of the positive pole, and extending almost to the

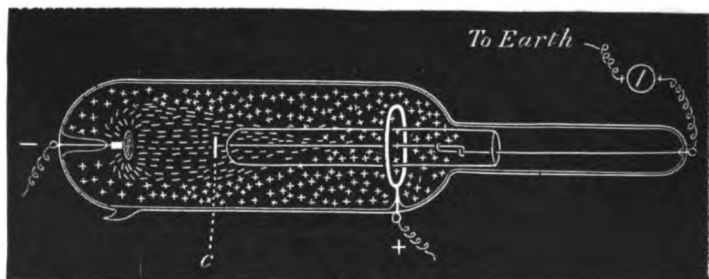
negative, the tube is strongly electrified with positive electricity, the negative atoms shooting out from the negative pole in a rapidly diminishing cone. If an idle pole is placed in the position shown at Fig. 13, *a*, the impacts of positive and negative molecules are about equal, and no decided current will pass from it, through the galvanometer, to earth. This is the *neutral point*. But if we imagine the idle pole to be as at Fig. 13, *b*, then the

$$\begin{aligned} P &= 0.0001 \text{ mm.} \\ &= 0.18 \text{ M.} \end{aligned}$$

FIG. 13, *b*.

positively electrified molecules greatly preponderate over the negative molecules, and positive electricity is shown. If the idle pole is now shifted as shown at Fig. 13, *c*, the negative molecules preponderate, and the pole will give negative electricity.

$$\begin{aligned} P &= 0.0001 \text{ mm.} \\ &= 0.18 \text{ M.} \end{aligned}$$

FIG. 13, *c*.

As the exhaustion proceeds, the positive charge in the tube increases, and the neutral point approaches closer to the negative pole, and at a point just short of non-conduction, so greatly does the

positive electrification preponderate that it is almost impossible to get negative electricity from the idle pole, unless it actually touches the negative pole. This tube is before you, and I will now proceed to show the change in direction of current by moving the idle pole.

I have not succeeded in getting the "Edison" current in incandescent lamps to change in direction at even the highest degree of exhaustion which my pump will produce. The subject requires further investigation, and, like other residual phenomena, these discrepancies promise a rich harvest of future discoveries to the experimental philosopher, just as the waste products of the chemist have often proved the source of new and valuable bodies.

#### PROPERTIES OF RADIANT MATTER.

One of the most characteristic attributes of radiant matter—whence its name—is that it moves in approximately straight lines, and in a direction almost normal to the surface of the electrode. If we keep the induction current passing continuously through a vacuum tube in the same direction, we can imagine two ways in which the action proceeds: either the supply of gaseous molecules at the surface of the negative pole must run short, and the phenomena come to an end, or the molecules must find some means of getting back. I will show you an experiment which

$$P = 0.001 \text{ mm.} \\ = 1.3 \text{ M.}$$

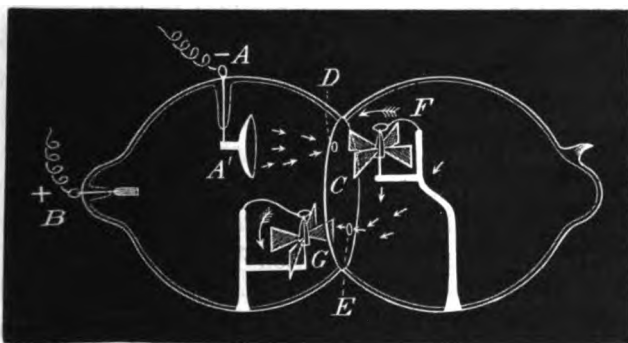


FIG. 14.

reveals the molecules in the very act of returning. Here is a tube (Fig. 14), exhausted to a pressure of 0.001 mm., or 1.3 M. In

the middle of the tube is a thin glass diaphragm, C, pierced with two holes, D and E. At one part of the tube a concave pole, A', is focussed on the upper hole, D, in the diaphragm. Behind the upper hole and in front of the lower one are movable vanes, F and G, capable of rotation by the slightest current of gas through the holes.

On passing the current with the concave pole negative, the small vanes rotate in such a manner as to prove that at this high exhaustion a stream of molecules issues from the lower hole in the diaphragm, whilst at the same time a stream of freshly charged molecules is forced by the negative pole through the upper hole. The experiment speaks for itself, showing as forcibly as an experiment can show that so far the theory is right.

This view of the ultra-gaseous state of matter is advanced merely as a working hypothesis, which, in the present state of our knowledge, may be regarded as a necessary help, to be retained only so long as it proves useful. In experimental research early hypotheses have necessarily to be modified, or adjusted, or perhaps entirely abandoned, in deference to more accurate observations. Dumas said truly that hypotheses were like crutches, which we throw away when we are able to walk without them.

#### RADIANT MATTER AND "RADIANT ELECTRODE MATTER."

In recording my investigations on the subject of radiant matter and the state of gaseous residues in high vacuum under electrical strain, I must refer to certain attacks on the views I have propounded. The most important of these questionings are contained in a volume of "Physical Memoirs," selected and translated from foreign sources, under the direction of the Physical Society (vol. i., part 2). This volume contains two memoirs—one by Hittorff, on the "Conduction of Electricity in Gases," and the other by Puluj, on "Radiant Electrode Matter and the So-called Fourth State." Dr. Puluj's paper concerns me most, as the author has set himself vigorously to the task of opposing my conclusions. Apart from my desire to keep controversial matter out of an address of this sort, time would not permit me to discuss the points raised by my critic; I will

therefore only observe in passing that Dr. Puluĵ has no authority for linking my theory of a fourth state of matter with the highly transcendental doctrine of four-dimensional space.

Reference has already been made to the mistaken supposition that I have pronounced the thickness of the dark space in a highly exhausted tube, through which an induction spark is passed, to be identical with the natural mean free path of the molecules of gas at that exhaustion. I could quote numerous passages from my writings to show that what I meant, and said, was the mean free path as amplified and modified by the electrification.\* In this view I am supported by Professor Schuster,† who, in a passage quoted below, distinctly admits that the mean free path of an electrified molecule may differ from that of one in its ordinary state.

The great difference between Puluĵ and me lies in his statement that "*the matter which fills the dark space consists of*

\* "The thickness of the dark space surrounding the negative pole is the measure of the mean length of the path of the gaseous molecules between successive collisions. The electrified molecules are projected from the negative pole with enormous velocity, varying, however, with the degree of exhaustion and intensity of the induction current."—*Phil. Trans.*, part i., 1879, par. 530.

"The extra velocity with which the molecules rebound from the excited negative pole keeps back the more slowly moving molecules which are advancing towards the pole. The conflict occurs at the boundary of the dark space, where the luminous margin bears witness to the energy of the discharge."—*Phil. Trans.*, part i., 1879, par. 507.

"Here, then, we see the induction spark actually illuminating the lines of molecular pressure caused by the excitement of the negative pole."—R.I. Lecture, Friday, April 4th, 1879.

"The electrically excited negative pole supplies the *force majeure* which entirely, or partially, changes into a rectilinear action the irregular vibration in all directions."—*Proc. Roy. Soc.*, 1880, page 472.

"It is also probable that the absolute velocity of the molecules is increased so as to make the mean velocity with which they leave the negative pole greater than that of ordinary gaseous molecules."—*Phil. Trans.*, part ii., 1881, par. 719.

† "It has been suggested that the extent of the dark space represents the mean free path of the molecules. . . . It has been pointed out by others that the extent of the dark space is really considerably greater than the mean free path of the molecules, calculated according to the ordinary way. My measurements make it nearly twenty times as great. This, however, is not in itself a fatal objection; for, as we have seen, the mean free path of an ion may be different from that of a molecule moving among others."—Schuster, *Proc. Roy. Soc.*, xlvii., pp. 556, 557.

*"mechanically detached particles of the electrodes which are charged with statical negative electricity, and move progressively in a straight direction."*\*

To these mechanically detached particles of the electrodes, "of different sizes, often large lumps"† Puluĵ attributes all the phenomena of heat, force, and phosphorescence that I from time to time have described in my several papers.

Puluĵ objects energetically to my definition, "radiant matter," and then proposes in its stead the misleading term, "radiant electrode matter." I say "misleading," for while both his and my definitions equally admit the existence of "radiant matter," he drags in the hypothesis that the radiant matter is actually the disintegrated material of the poles.

Puluĵ declares that the phenomena I have described in high vacua are produced by his irregularly shaped lumps of "radiant electrode matter." My contention is that they are produced by radiant matter of the residual molecules of gas.

Were it not that in this case we can turn to experimental evidence, I would not mention the subject to you. On such an occasion as this controversial matter must have no place; therefore I content myself, at present, by showing a few novel experiments which demonstratively prove my case.

Let me first deal with the "radiant electrode" hypothesis. Some metals, it is well known—such as silver, gold, or platinum—when used for the negative electrode in a vacuum tube, volatilise more or less rapidly, coating any object in their neighbourhood with a very even film. On this depends the well-known method of electrically preparing small mirrors, &c. Aluminium, however, seems exempt from this volatility. Hence, and for other reasons, it is generally used for electrodes.

If, then, the phenomena in a high vacuum are due to the "electrode matter," the more volatile the metal used, the greater should be the effect.‡

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\* "Physical Memoirs," part 2., vol. i., page 244. The paragraph is italicised in the original.

† *Lec. cit.*, p. 242.

‡ In a valuable paper read before the Royal Society, November 20th, 1890, by

Here is a tube (Fig. 15— $P = 0.00068$  mm., or 0.9 M.) with two negative electrodes, A, A', so placed as to project two

$$P = 0.00068 \text{ mm.} \\ = 0.9 \text{ M.}$$

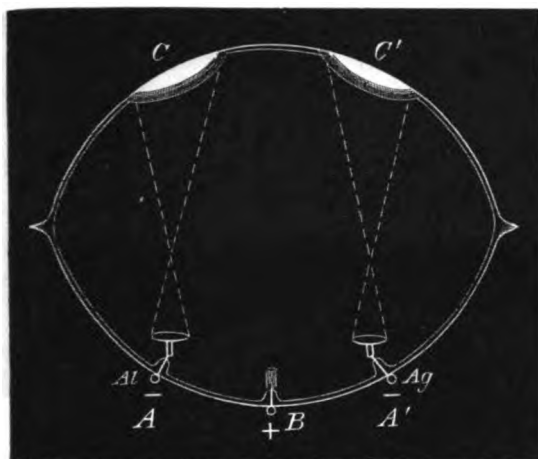


FIG. 15.

luminous spots on the phosphorescent glass of the tube. One electrode, A', is of pure silver, a volatile metal; the other, A, is of aluminium, practically non-volatile. A quantity of "electrode matter" will be shot off from the silver pole, and practically none from the aluminium pole; but you see that in each case the phosphorescence, C, C', is identical. Had the "radiant electrode matter" been the active agent, the more intense phosphorescence would proceed from the more volatile pole.

A drawing of another experimental piece of apparatus is shown in Fig. 16. A pear-shaped bulb of German glass has near the small end an inner concave negative pole, A, of pure silver, so mounted that its inverted image is thrown upon the opposite end of the tube. In front of this pole is a screen of mica, C, having a small hole in the centre, so that only a narrow pencil of

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Professors Liveing and Dewar, on finely divided metallic dust thrown off the surface of various electrodes in vacuum tubes, they find, not only that dust, however fine, suspended in a gas will not act like gaseous matter in becoming luminous with its characteristic spectrum in an electric discharge, but that it is driven with extraordinary rapidity out of the course of the discharge.

rays from the silver pole can pass through, forming a bright spot, D, at the far end of the bulb. The exhaustion is about the same:

$$\begin{aligned} P &= 0.00068 \text{ mm.} \\ &= 0.9 \text{ M.} \end{aligned}$$

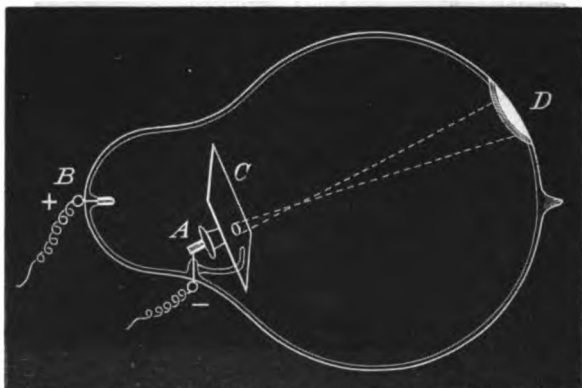


FIG. 16.

as in the previous tube, and the current has been allowed to pass continuously for many hours, so as to drive off a certain portion of the silver electrode; and upon examination it is found that the silver has all been deposited in the immediate neighbourhood of the pole, whilst the spot D, at the far end of the tube, that has been continuously glowing with phosphorescent light, is practically free from silver.

The experiment is too lengthy for me to repeat it here, so I shall not attempt it, but I have on the table the results for examination.

The identity of action of silver and aluminium in the first case, and the non-projection of silver in this second instance, in themselves are sufficient to condemn Dr. Puluj's hypothesis, since they prove that phosphorescence is independent of the material of the negative electrode. In front of me is a set of tubes that, to my mind, puts the matter wholly beyond doubt. The tubes contain no inside electrodes with the residual gaseous molecules, and with them I will proceed to give some of the most striking "radiant matter" experiments without any inner metallic poles at all.

In all these tubes the electrodes, which are of silver, are on the outside, the current acting through the body of the glass. The first tube contains gas only slightly rarefied, and at the stratification stage. It is simply a closed glass cylinder, with a coat of silver deposited outside at each end, and exhausted to a

$$P = 2.0 \text{ mm.}$$

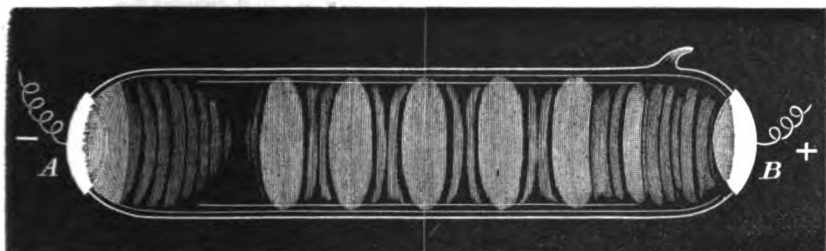


FIG. 17.

pressure of 2 mm. The outline of the tube is shown in Fig. 17. I pass a current, and, as you see, the stratifications, though faint, are perfectly formed.

$$P = 0.076 \text{ mm.} \\ = 100.0 \text{ M.}$$

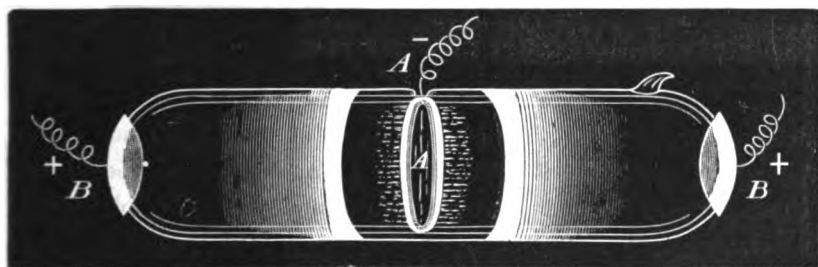


FIG. 18.

The next tube, seen in outline in Fig. 18, shows the dark space. Like the first, it is a closed cylinder of glass, with a central indentation forming a kind of hanging pocket, and almost dividing the tube into two compartments. This pocket, silvered on the air side, forms a hollow glass diaphragm that can be connected electrically from the outside, forming the negative pole, A; the two ends of the tube, also outwardly silvered, form the positive poles, B, B. I pass the current, and you all see the

dark space distinctly visible. The pressure here is 0.076 mm., or 100 M.

The next stage, dealing with more rarefied matter, is that of phosphorescence. Here is an egg-shaped bulb, shown in Fig. 19,

$$P = 0.00068 \text{ mm.} \\ = 0.9 \text{ M.}$$

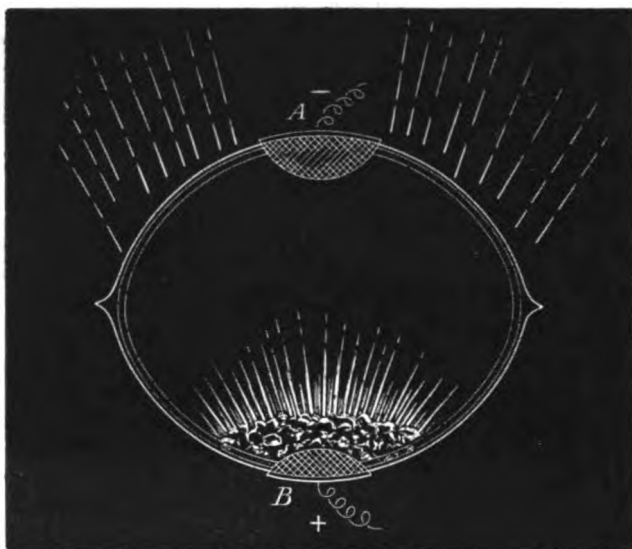


FIG. 19.

containing some pure yttria, and a few rough rubies. The positive electrode, B, is on the bottom of the tube, under the phosphorescent material; the negative, A, is on the upper part of the tube. See how well the rubies and yttria phosphoresce under molecular bombardment, at an internal pressure of 0.00068 mm., or 0.9 M.

A shadow of an object inside a bulb can also be projected on to the opposite wall of the bulb by means of an outside pole. A mica cross is supported in the middle of the bulb (Fig. 20), and on connecting a small silvered patch, A, on one side of the bulb with the negative pole of the induction coil, and putting the positive pole to another patch of silver, B, at the top, the opposite side of the bulb glows with a phosphorescent light, on which the black shadow of the cross seems sharply cut out. Here the internal pressure is 0.00068 mm., or 0.9 M.

Passing to the next phenomenon, I proceed to show the production of mechanical energy in a tube without internal poles.

$$P = 0.00068 \text{ mm.} \\ = 0.9 \text{ M.}$$

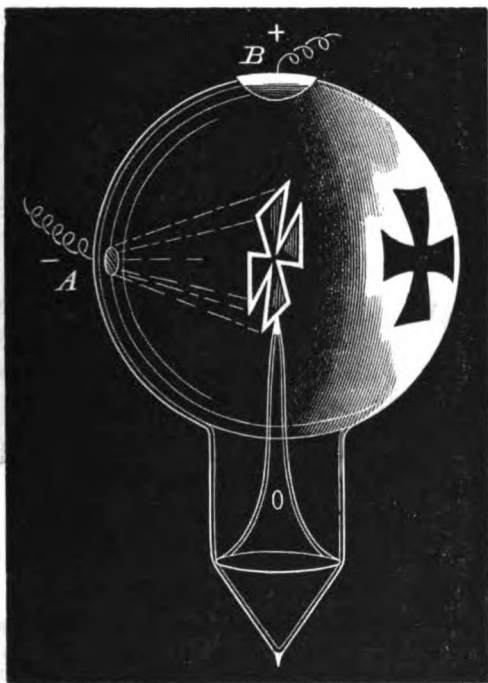


FIG. 20.

It is shown in Fig. 21 ( $P = 0.001 \text{ mm.}$ , or  $1.3 \text{ M.}$ ). It contains a light wheel of aluminium, carrying vanes of transparent mica, the poles, A, B, being in such a position outside that the molecular focus falls upon the vanes on one side only. The bulb is placed in the lantern, and the image is projected on the screen: if I now pass the current, you see the wheel rotates rapidly, reversing in direction as I reverse the current.

Here is an apparatus (Fig. 22) which shows that the residual gaseous molecules when brought to a focus produce heat. It consists of a glass tube with a bulb blown at one end, and a small bundle of carbon wool, C, fixed in the centre, and exhausted to pressure of  $0.000076 \text{ mm.}$ , or  $0.1 \text{ M.}$  The negative electrode, A, is formed by coating part of the outside of the bulb with silver,

$P = 0.001 \text{ mm}$   
 $= 1.8 \text{ M.}$

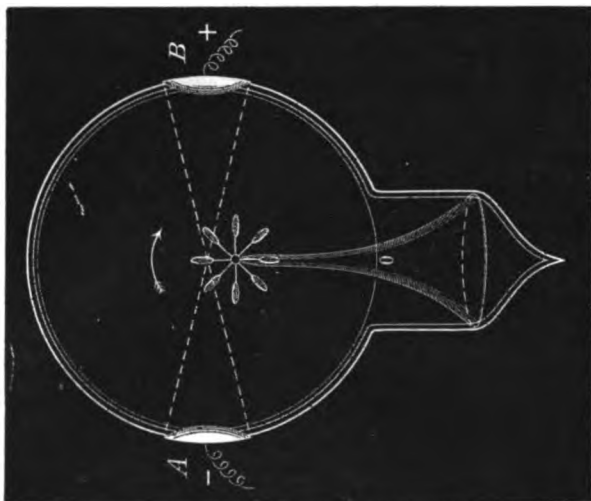


Fig. 21

$P = 0.000076 \text{ mm.}$   
 $= 0.1 \text{ M.}$

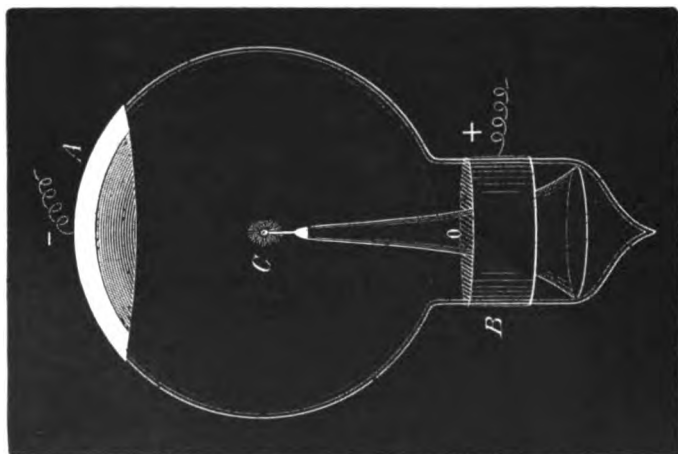


Fig. 22.

and it is in such a position that the focus of rays falls upon the carbon wool. The positive electrode, B, is an outer coating at the other end of the tube. I pass the current, and those who are close may see the bright sparks of carbon raised to incandescence by the impact of the molecular stream.

You thus have seen that all the old "radiant matter" effects can be produced in tubes containing no metallic electrodes to volatilise. It may be suggested that the sides of the tube in contact with the outside poles become electrodes in this case, and that particles of the glass itself may be torn off and projected across, and so produce the effects. This is a strong argument, which fortunately can be tested by experiment. In the case of this tube (Fig. 23— $P = 0.00068$  mm., or 0.9 M.) the bulb is made

$$\begin{aligned} P &= 0.00068 \text{ mm.} \\ &= 0.9 \text{ M.} \end{aligned}$$

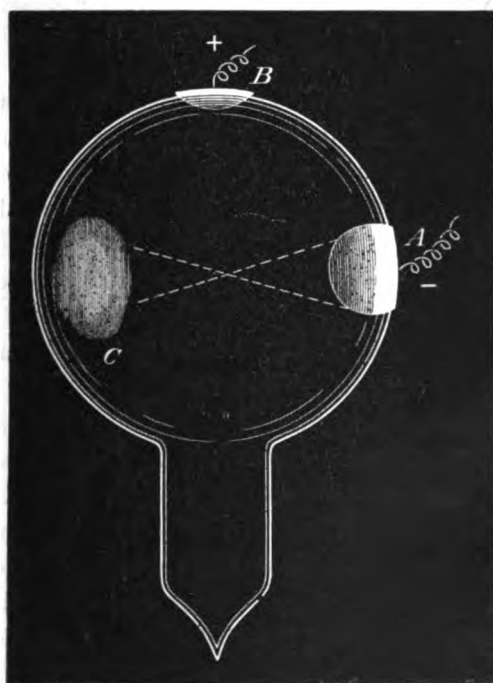


FIG. 23.

of lead glass, phosphorescing blue under molecular bombardment. Inside the bulb, completely covering the part that would form the

negative pole, A, I have painted a substantial coat of yttria, so as to interpose a layer of this earth between the glass and the inside of the tube. The negative and positive poles are silver disks on the outside of the bulb, A being the negative and B the positive pole. If, therefore, particles are torn off and projected across the tube to cause phosphorescence, these particles will not be particles of glass, but of yttria, and the spot of phosphorescent light, C, on the opposite side of the bulb will not be the dull blue of lead glass, but the golden yellow of yttria. You see there is no such indication; the glass phosphoresces with its usual blue glow, and there is no evidence that a single particle of yttria is striking it.

Witnessing these effects, I think you will agree I am justified in adhering to my original theory that the phenomena are caused by the radiant matter of the residual gaseous molecules, and certainly not by the torn-off particles of the negative electrode.

#### PHOSPHORESCENCE IN HIGH VACUA.

I have already pointed out that the molecular motions rendered visible in a vacuum tube are not the motions of molecules under ordinary condition, but are compounded of these ordinary, or kinetic, motions and the extra motion due to the electrical impetus.

Experiments show that in such tubes a few molecules may traverse more than a hundred times the *mean* free path, with a correspondingly increased velocity, until they are arrested by collisions. Indeed, the molecular free path may vary in one and the same tube, and at one and the same degree of exhaustion.

Very many bodies, such as ruby, diamond, emerald, alumina, yttria, samaria, and a large class of earthy oxides and sulphides, phosphoresce in vacuum tubes when placed in the path of the stream of electrified molecules proceeding from the negative pole. The composition of the gaseous residue present does not affect phosphorescence; thus, the earth yttria phosphoresces well in the residual vacua of atmospheric air, of oxygen, nitrogen, carbonic anhydride, hydrogen, iodine, sulphur, and mercury.

With yttria in a vacuum tube, the point of maximum phosphorescence, as I have already pointed out, lies on the margin of

the dark space. The diagram (Fig. 24) shows approximately the degree of phosphorescence in different parts of a tube at an inter-

$$P = 0.25 \text{ mm.} \\ = 330.0 \text{ M.}$$

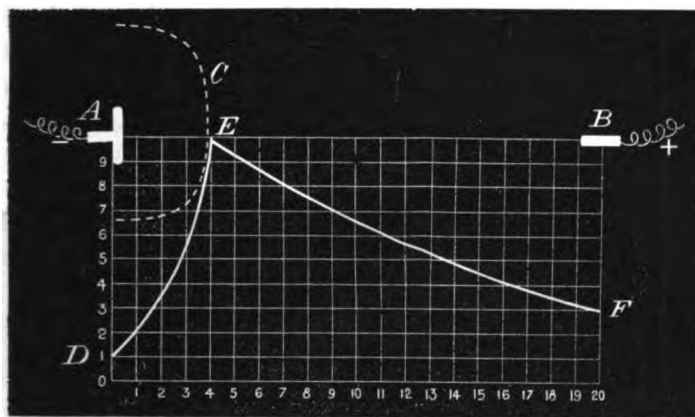


FIG. 24.

nal pressure of 0.25 mm., or 330 M. On the top you see the positive and negative poles, A and B, the latter having the outline of the dark space shown by a dotted line, C. The curve D E F shows the relative intensities of the phosphorescence at different distances from the negative pole, and the position inside the dark space at which phosphorescence does not occur. The height of the curve represents the degree of phosphorescence. The most decisive effects of phosphorescence are reached by making the tube so large that the walls are outside the dark space, whilst the material submitted to experiment is placed just at the edge of the dark space.

Hitherto I have spoken only of phosphorescence of substances placed under the negative pole. But from numerous experiments I find that bodies will phosphoresce in actual contact with the negative pole.

This is only a temporary phenomenon, and ceases entirely when the exhaustion is pushed to a very high point. The experiment is one scarcely possible to exhibit to an audience, so I must content myself with describing it. A U-tube (shown in Fig. 25) has a flat aluminium pole, in the form of a disk, at each end,

both coated with a paint of phosphorescent yttria. As the rarefaction approaches about 0.5 mm. the surface of the negative pole, A, becomes faintly phosphorescent. On continuing the exhaustion this luminosity rapidly diminishes, not only in intensity, but in extent, contracting more and more from the edge of the disk, until ultimately it is visible only as a bright spot in the centre. This fact does not prop a recent theory, that as the exhaustion gets higher the discharge leaves the centre of the pole, and takes place only between the edge and the walls of the tube.

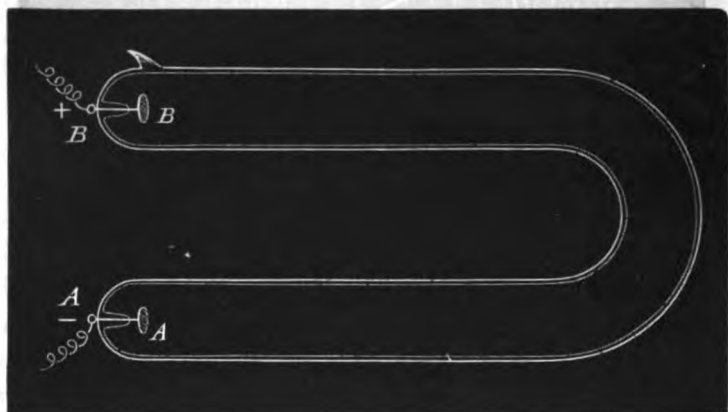


FIG. 25.

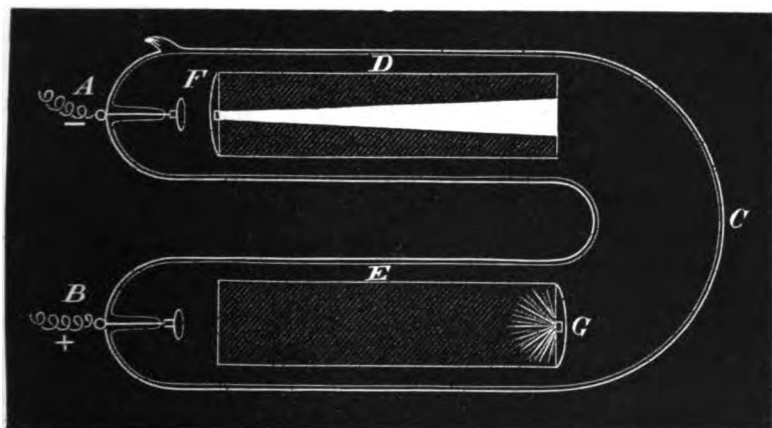
If the exhaustion is further pushed, then at the point where the surface of the negative pole ceases to be luminous the material on the positive pole, B, commences to phosphoresce, increasing in intensity until the tube refuses to conduct, its greatest brilliancy being just short of this degree of exhaustion. The probable explanation is that the vagrant molecules I introduce in the next experiment, happening to come within the sphere of influence of the positive pole, rush violently to it and excite phosphorescence in the yttria, whilst losing their negative charge.

#### LOOSE AND ERRATIC MOLECULES.

In the brief time left to me this evening I cannot touch upon the mass of experiments made to render this result clear, so I will at once show you a piece of apparatus that clearly illustrates

the cause of phosphorescence at the positive pole. A drawing of this tube is shown at Fig. 26, but let me first explain the effect I expect to obtain, and then endeavour to show the actual experiment.

$$P = 0.076 \text{ mm.} \\ = 100.0 \text{ M.}$$



$$P = 0.000076 \text{ mm.} \\ = 0.1 \text{ M.}$$

FIG. 26.

A C B is a U-shaped tube with terminals, A and B, at each end; D and E are two mica screens covered with a phosphorescent powder, having at F and G other screens with a small slit in front, so as to allow only a narrow beam of charged molecules to pass through. At first the tube is exhausted to a pressure of 0.076 mm., or 100 M., and you see how sharp and slightly divergent is the luminous image on screen D, whilst not a trace of phosphorescence is to be seen on the screen E in the other limb of the tube. Now I push the exhaustion to the highest point short of non-conduction (0.000076 mm., or 0.1 M.), and the phenomena change. The initial line of light on D becomes wider and unsteady, whilst at the gate, G, a decided phosphorescence is observable entering the second screen, E. This luminosity diverges to a considerable extent, much more than did the stream of charged molecules observed in the first limb of the tube at the lower exhaustion. This is only to be expected, inasmuch as those sparse molecules which have run the gauntlet

of the crowd and have been hunted and buffeted round the corner will have little or no lateral support, but wander to the furthest part of the tube, and altogether behave differently to the orderly procession of molecules at more moderate exhaustion.

### THE RESISTANCE OF HIGH VACUA.

I must only lightly touch this phenomenon to-night; it is a subject of deep interest, and one to which I have lately devoted much time. Shortly I hope to publish a full account of results recently obtained.

The passage of an induction current at a high vacuum through a tube depends much upon the material of the tube or the substance enclosed within it. Given the same degree of exhaustion, and the same distance between the terminals inside the tube, the E.M.F. necessary to force the current through may vary from 3,000 volts to 20,000 volts, according to the particular material used.

Here is a very striking example that will serve to illustrate this phenomenon. Fig. 27 is a double tube joined by a narrow

$$P = 0.02 \text{ mm.} \\ = 26.0 \text{ M.}$$

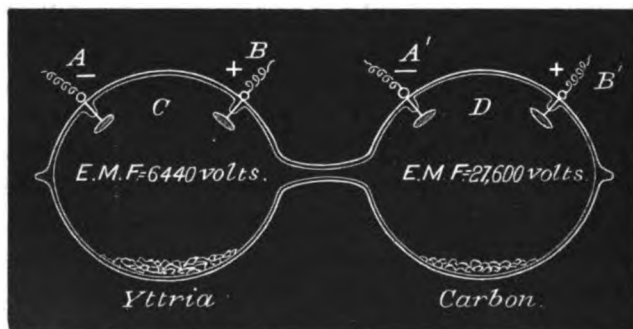


FIG. 27.

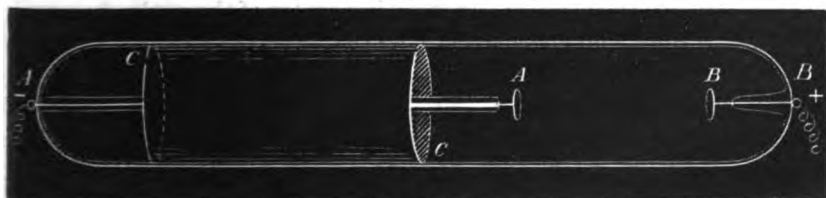
open channel, and therefore in the same state of exhaustion throughout ( $P = 0.02$  mm., or 26 M.). Each tube is furnished with a pair of poles, A, B, and A', B'. One tube contains the phosphorescent earth yttria, the other contains finely divided carbon. I first connect the yttria tube, C, to the coil, and place in parallel circuit with it a spark gauge. To begin with, this

gauge is set to a gap of 1 mm. (An E.M.F. of 920 volts is necessary to strike across 1 mm. of air; therefore the difference of potential at the terminals inside the yttria tube is 920 volts, and this, you perceive, is not sufficient to force the current through the tube.) I now gradually open the gauge until the potential of the inner terminals has risen to a point high enough to allow the spark to pass through the tube, making the yttria phosphorescent.

The gap is at 7 mm. equal to an E.M.F. of 6,440 volts. I next attach the coil wires to the tube D, containing the carbon, and the exhaustion in the tubes being the same as before, I repeat the experiment: you now see that the gap has to be opened to 30 mm., equivalent to an E.M.F. of 27,600 volts, before the current will pass through the tube. The fact of whether the vacuum tube contains yttria or carbon makes a difference of 21,160 volts in the E.M.F. required to cause a discharge between the terminals.

One other experiment I would like to show in further illustration of this resistance. The idea suggested itself that possibly differences in the material or conductivity of the particular bulbs might influence the results you have just seen. Here (Fig. 28) is a long cylindrical tube of phosphorescent

$$\begin{aligned} P &= 0.00068 \text{ mm.} \\ &= 0.9 \text{ M.} \end{aligned}$$



Silver Cylinder off Poles, E.M.F. = 1,380 volts.  
 " " round Poles, E.M.F. = 6,440 volts.

FIG. 28.

Bohemian glass containing a pair of terminals, A, B; it also contains a shorter cylinder of glass, C C, brightly silvered inside. The internal pressure is 0.00068 mm., or 0.9 M. The silver cylinder is now at one end of the tube, out of the way of the

terminals, which therefore have phosphorescent glass around them. I turn on the coil, and find that the E.M.F. necessary to force the current through, now that the terminals are in a phosphorescent chamber, is 1,380 volts. I slide the cylinder down to the end of the tube, so as to inclose the terminals in a metallic silver chamber, and you see the E.M.F. necessary to pass the current rises to 6,440 volts. Metallic silver does not phosphoresce, whilst Bohemian glass phosphoresces very well. It appears that the greater the phosphorescing power of the substance surrounding the poles, so much the easier does the induction spark pass. Surround the poles with Bohemian glass or yttria—two phosphorescent non-conductors of electricity—and the induction spark passes easily; immediately I surround the terminals with a non-phosphorescent conductor, the current refuses to pass.

#### WHAT OCCASIONS PHOSPHORESCENCE?

I should like to interest you in a question that has exercised my mind for some time past. What is it that occasions the phosphorescence of yttria and other bodies *in vacuo* under molecular bombardment?

So far, I have found this phosphorescence to be an attribute of non-conductors only. We know that in the act of phosphorescence the molecules of yttria are in a state of intense vibration. Each molecule may be viewed as the radiating centre of the entire bundle of rays, which, when decomposed by the prism, displays a discontinuous spectrum. We may also suppose that the residual atoms of gas charged with negative electricity give off their electricity on coming in collision with a phosphorescent body, and on their return take up a fresh charge.

#### THE ELECTROLYSIS HYPOTHESIS.

There is a certain amount of evidence in favour of an electrolytic hypothesis of the passage of electricity through rarefied gases. This has been ably advocated by Professor Schuster in the Bakerian Lecture before the Royal Society, March 20th, 1890.\*

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\* *Roy. Soc. Proc.*, xlvii., 526.

A molecule of hydrogen gas, for instance, may be made up of one group of atoms of hydrogen having an equivalent of negative electricity inherent in it, and one group of atoms having an equivalent of positive electricity bound up with it. These atoms are also charged with *additional* equivalents of positive or negative electricity, which they carry about as a ship carries its cargo. We are not concerned with the inherent electricity—of which we are ignorant—but with the extra, or “cargo,” charge. Let us imagine a molecule of hydrogen near the face of the negative pole in a vacuum tube. I turn on the current, and the atoms of the hydrogen molecule are dragged apart. The positive atom is attracted to the negative pole, where the violence of impact, or the discharge of electricity, renders it apparent with evolution of light. The internal luminous layer, which is closely adjacent to the negative electrode, is due, therefore, to the positive atoms rushing to the negative pole, and not, like the glow round the edge of the dark space, to the negative atoms projected from it. The negative atom, on the other hand, is driven violently from the negative pole, in virtue of the mutual repulsion existing between any two bodies similarly electrified, with a velocity varying with the intensity of the electrification and the degree of the vacuum; the more perfect the vacuum the greater the velocity, the atoms flying outwards in straight lines until they meet with an obstacle. Such an obstacle may be a procession of positively charged atoms from the positive pole: in this case the two kinds of atoms mutually discharge each other’s cargos with a display of light. This phenomenon occurs at the margin of the dark space when the vacuum is only moderate. Or the obstacle may be produced by the vacuum being so high that the atoms of gas present are too few to form a continuous procession. (*Why* a high vacuum should be non-conductive does not clearly appear, but the fact itself is beyond doubt; it is probably connected with the inability of electrified atoms to leave the poles.) Or, again, the obstacle may be a phosphorescent body like yttria. In this case the negatively charged atoms deliver up their charge of electricity to the yttria, which is so constituted—perhaps after the manner of a Hertz resonator—that its atoms charge and

discharge, vibrating about 550 billion times in a second, and producing waves in the ether of the length, approximately, of 5·74 ten-millionths of a millimetre, and occasioning in the eye the effect of citron light.

We are not under the necessity of supposing that this number of hydrogen atoms are driven against the yttria in the second, although even at a high vacuum there are quite enough atoms left in the bulb to keep up such a supply. All that is needed is that a succession of shocks, not necessarily rhythmical, may strike the yttria at frequencies which will set up such a number of vibrations, just as a series of slow impacts on a gong causes it to emit sound waves of much greater frequency.

In a low vacuum only very few atoms can run the gauntlet among the crowd of intruding atoms, and those few which succeed in reaching the yttria arrive with much reduced velocity, and so faint is the phosphorescence they set up that it is completely obscured\* by the brighter phosphorescence of the residual gas. As the vacuum becomes higher, more and more atoms find their way across, and their speed being at the same time accelerated, the phosphorescence becomes intensified.

At a good vacuum most of the atoms hit the yttria, their velocity is increased, and the rhythmical excitation reaches its maximum.

#### THE DARK SPACE IN MERCURY VAPOUR.

In applying the electrolytic hypothesis, I have used for illustration's sake the gaseous residue of hydrogen, which is known as a diatomic gas. I have found, however, the phenomena of the dark space, &c., to occur in the vapour of mercury, which is a monatomic gas. This important result induced me to patiently investigate this subject, and the result of one experiment is before you (Fig. 29). The tube is furnished with aluminium terminals, and is so arranged that the induction spark can be kept passing during exhaustion, to drive off occluded gases. When at the highest attainable vacuum, the tube is filled with pure mercury

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\* This faint phosphorescence at a low vacuum can be rendered visible by the electrical phosphoroscope described in my lecture at the Royal Institution in 1887.

by simply raising a reservoir. On applying heat the entire contents of the tube are boiled away and pass down the fall tubes of

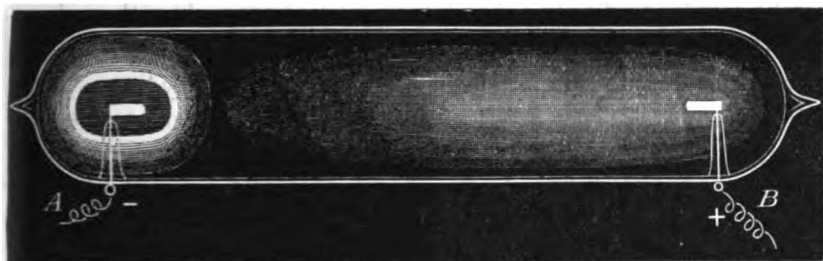


FIG. 29.

the pump, exhaustion going on at the same time. When the whole of the mercury has thus been boiled away *in vacuo*, except a little condensed at the upper part of the tube, the results on passing the spark are as follows:—When the tube is cold, the induction current refuses to pass; on gently heating with a gas-burner, the current passes, and the dark space is distinctly visible. Continuing the heat so as to volatilise the drops of mercury condensed on the sides, the whole tube becomes filled with a green phosphorescent light, the dark space gets smaller and smaller, and ultimately the negative pole becomes covered with a luminous glow. On allowing the tube to cool, the same phenomena ensue in inverse order. The luminous halo expands, showing the dark space between it and the pole, and this dark space gradually grows larger as the tube becomes cooler; the mercury again condenses on the side of the tube, the green phosphorescence grows paler and paler, until at last the induction spark from the large coil refuses to pass.

At first sight this result appears fatal to the electrolytic hypothesis, for if the molecule of mercury contains only one atom, how can we talk of its separation into positive and negative atoms by the electric stress? It must be remembered, however, that we are as yet ignorant of the absolute mass of the atom of any element. All that can be said is that a molecule of free hydrogen becomes halved in combining chemically with certain other elements, whilst a molecule of free mercury does not suffer division on yielding any known compound of mercury: the physical atoms in the one

behave as two separate groups, and those in the other as one undivided group. It has been agreed by chemists, for simplicity's sake, and for facilitating chemical calculations, to reduce the units to the lowest term, consistent with the avoidance of fractions: we therefore say that the atoms in a molecule of free hydrogen act in chemistry as two separate groups, each of a minimum relative weight of 1, whilst those in a molecule of free mercury act as one undivided group of the relative weight 200. But to what number of atoms the 1 and 200 correspond respectively no chemist knows.

To show how intimately chemistry and electricity interlock, I may here remark that one of the latest theories in chemistry renders such a division of the molecule into groups of electro-positive and electro-negative atoms necessary for a consistent explanation of the genesis of the elements. This is so important that I may be excused for digressing a little into this development of theoretical chemistry.

#### GENESIS OF THE ELEMENTS.

It is now generally acknowledged that there are several ranks in the elemental hierarchy, and that besides the well-defined groups of chemical elements, there are underlying sub-groups. To these sub-groups has been given the name of "meta-elements." The original genesis of atoms assumes the action of two forms of energy working in time and space—one operating uniformly in accordance with a continuous fall of temperature, and the other having periodic cycles of ebb and swell, and intimately connected with the energy of electricity (Fig. 30). The centre of this creative force in its journey through space scattered seeds, or sub-atoms, that ultimately coalesced into the groupings known as chemical elements. At this genetic stage the new-born particles vibrating in all directions and with all velocities, the faster-moving ones would still overtake the laggards, the slower would obstruct the quicker, and we should have groups formed in different parts of space. The constituents of each group whose form of energy governing atomic weight was not in accord with the mean rate of the bulk of the components of that group, would work to the outside and be thrown off to

find other groups with which they were more in harmony. In time a condition of stability would be established, and we should

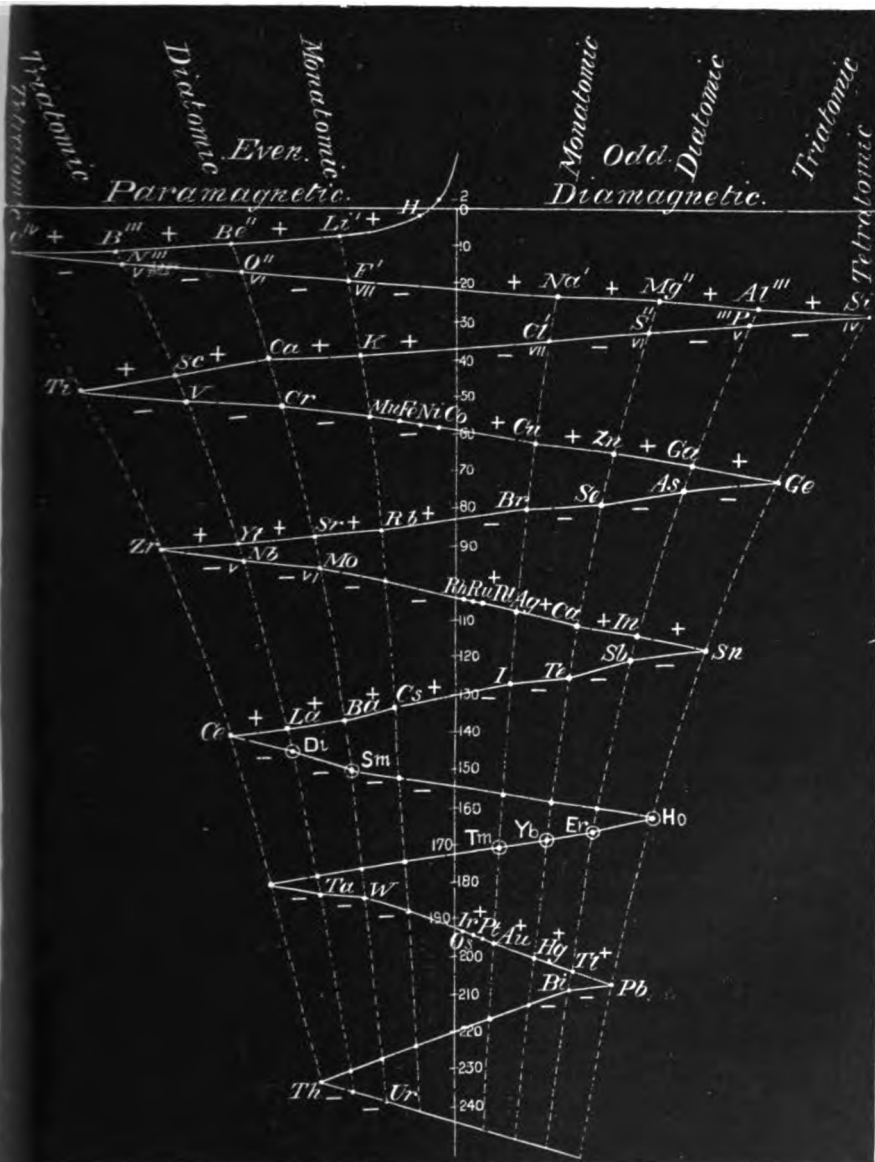


FIG. 30.

have our present series of chemical elements, each with a definite

atomic weight—definite on account of its being the average weight of an enormous number of sub-atoms, or meta-elements, each very near to the mean. The atomic weight of mercury, for instance, is called 200, but the atom of mercury, as we know it, is assumed to be made up of an enormous number of sub-atoms, each of which may vary slightly round the mean number 200 as a centre.

We are sometimes asked why, if the elements have been evolved, we never see one of them transformed, or in process of transformation, into another. The question is as futile as the cavil that in the organic world we never see a horse metamorphosed into a cow. Before copper, *e.g.*, can be transmuted into gold, it would have to be carried back to a simpler and more primitive state of matter, and then, so to speak, shunted on to the track which leads to gold.

This atomic scheme postulates a to-and-fro motion of a form of energy governing the electrical state of the atom. It is found that those elements generated as they approach the central position are electro-positive, and those on the retreat from this position are electro-negative. Moreover, the degree of positiveness or negativeness depends on the distance of the element from the central line; hence, calling the atom in the mean position electrically neutral, those sub-atoms which are on one side of the mean will be charged with positive electricity, and those on the other side of the mean position will be charged with negative electricity, the whole atom being neutral.

This is not a mere hypothesis, but may take the rank of a theory. It has been experimentally verified as far as possible with so baffling an enigma. Long-continued research in the laboratory has shown that in matter which has responded to every test of an element, there are minute shades of difference which have admitted of selection and resolution into meta-elements, having exactly the properties required by theory. The earth yttria, which has been of such value in these electrical researches as a test of negatively excited atoms, is of no less interest in chemistry, having been the first body in which the existence of this sub-group of meta-elements was demonstrated.

## CONCLUSION.

I frankly admit I have by no means exhausted the subject which daily and nightly fills my thoughts. I have ardently sought for facts on which to base my theory. I have struggled with problems which must be conquered before we can arrive at exact conclusions—conclusions which, so far as inorganic Nature is concerned, can only be reached by the harmonious interfusion—not confusion—of our present twin sciences, electricity and chemistry. Of this interfusion I have just endeavoured to give you a foretaste. In elaborating the higher physics, the study of electrical phenomena must take a large, perhaps the largest, share.

We have invaded regions once unknown, but a formidable amount of hard work remains to be completed. As we proceed we may look to electricity not only to aid, as it already does, our sense of hearing, but to sharpen and develop other powers of perception.

Science has emerged from its childish days. It has shed many delusions and impostures. It has discarded magic, alchemy, and astrology. And certain pseudo-applications of electricity, with which the present Institution is little concerned, in their turn will pass into oblivion.

There is no occasion to be disheartened at the apparent slow pace of elemental discovery. The desponding declare that if Roger Bacon could revisit “the glimpses of the moon,” he would shake his head to think we have got no further—that we are still in a haze as to the evolution of atoms. As for myself, I hold the firm conviction that unflagging research will be rewarded by an insight into natural mysteries such as now can scarcely be conceived. Difficulties, said a keen old statesman, are things to be overcome; and to my thinking Science should disdain the notion of finality. There is no stopping half-way, and we are resistlessly driven to ceaseless inquiry by the spirit “that impels all thinking” things, all objects of all thought, and rolls through all things.”

Sir WILLIAM THOMSON: We must not separate without expressing our thanks to the President for the admirable Address we have been listening to with such intense interest, and the

beautiful experiments with which he has illustrated it. To me it is particularly interesting to look back upon all this as the proceeds of a failure—no; I must correct myself—not a failure in the hands of Mr. Crookes, but what in the hands of most people would have been a dead failure. One evening sixteen years ago I had the privilege of seeing in his house what I believe was the very beginning of the series of researches which have led up to this grand development of physical science. Mr. Crookes told me he had been endeavouring to improve the Cavendish experiment by putting the movable balls into the receiver of an air pump, and removing the air, but he had failed. One of the difficulties encountered by Cavendish, and others who followed him in the experiment, had been disturbing currents of air produced by inequalities of temperature. It seemed natural to expect that these would be diminished by diminishing the density of the air. In air of ordinary density, the movable balls (as was well known to be the case with any delicately suspended small movable bodies screened from wind by a glass or metal enclosure) seemed to be attracted by a warm hand—a result correctly, no doubt, attributed to a flow of air towards the warmed part of the enclosing wall produced by the heat. This effect Mr. Crookes found to be diminished when the air was partially withdrawn by an air pump, but to his surprise he found reversed effects when the exhaustion was pushed to 2 or 3 millimetres of mercury. With very high exhaustions the seeming repulsions by warm bodies outside the enclosure became much more disturbing than the seeming attractions in full air; so Mr. Crookes was completely baffled in his attempt to improve the methods which had been followed by others in repeating Cavendish's experiment. But he went straight on to the radiometer. This is, indeed, one of the greatest discoveries that has ever been made in physical science, if we are to judge it by all it has done for the kinetic theory of gases, and by all the brilliant discoveries in electricity to which Mr. Crookes has been led by it. I can never forget how that night Mr. Crookes said to me, "I am going to try electrification." I was delighted, and I said, "Oh, certainly do; something great is sure to come of it." The

future historian of science, when he comes to write of the advance of knowledge and of thought in scientific matters which the nineteenth century has produced, will, I think, place—not second to Faraday, not second to Joule, but following both and crowning both—the discoverer of the radiometer and the passage of electricity “from plenum to vacuum.”

I beg to move—“That the cordial thanks of the Institution are due to the President for the brilliant Address just delivered by him, and that he be requested to permit its publication in the Journal of the Institution.”

Mr. PREECE: I will, with the permission of the meeting, second the resolution proposed by Sir William Thomson. Our President once paid me a very high compliment. He told me that it was always a pleasure to him to send his papers to me, because he knew I read them; and I can say this—that I believe there is no one single paper Professor Crookes has written that I have not only read, but deeply studied; and I am certain, from what you have heard to-night, that if any man in this room wants to acquire knowledge, whether of heat, of the kinetic theory of gases, or of radiant matter, he cannot do better than get a copy of the researches of our President, and study them with the same patience and care that I have. He says Science has emerged from its childhood. And who have caused it to do so but those great minds who, through this Victorian era, have been attacking these great questions? Sir William Thomson mentioned Faraday and Joule, and said Crookes would stand beside these. No doubt he will; but another name will stand there too, and that is the name of Sir William Thomson. Gentlemen, I beg, with your permission, to second this resolution.

The resolution, having been put by Sir William Thomson, was carried unanimously and enthusiastically.

The PRESIDENT: Sir William Thomson, Mr. Preece, ladies and gentlemen,—I feel overwhelmed by the flattering terms in which the proposer and seconder of this motion have spoken; I cannot take to myself one-tenth part of the credit given me. I must act somewhat as a shunt in respect of those thanks, and will take them in trust and distribute to those who deserve them

equally with myself. First of all, I must thank the authorities of the Royal College of Science for lending me the projection apparatus before you, and Professor Rücker for allowing his assistant, Mr. Chapman, to help me. To Mr. Chapman's skill and experience the success of the lantern and projection illustrations is due. I must not omit to acknowledge the great assistance I have had, not only in the preparation of this Address, but for many previous years, from my able assistant, Mr. Gardiner. He has worked late and early for this Address in the most indefatigable manner; he has made all the experimental tubes, has suggested improvements in the experiments, and, but for him, it would have been simply impossible for me to have put these illustrative experiments before you. A large portion of the thanks accorded me must therefore go to him. Then I have others to thank, but they might not like me to mention their names. They are here to-night, and are of a class of visitors who do not frequently grace these meetings with their presence. I will simply say that without the very complete series of diagrams before you, it would have been impossible to explain the abstruse points touched upon to-night. Now these diagrams were done in my house, although I was unable to prepare them myself. With your permission, then, I will distribute the thanks which you have so lavishly given me, in the proper quarters, and I thank you most heartily for what remains for myself.

The next meeting will take place on January 22nd, when a paper by Major-General Webber, Past-President, on "The Distribution of Electricity, with Special Reference to the Chelsea System," will be read.

A ballot for new members took place, at which the following candidates were elected :—

*Foreign Members :*

J. Carlos Calastremé.		Takeo Iwata, M.E.
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*Members :*

Sir John Coode, K.C.M.G.		Edmund Macrory, Q.C.
J. C. Graham.		Sir Richard Everard Webster,
Frederick Thomas Hollins.		Q.C., M.P.
Edgar Hughes.		

*Associates :*

Leonard Andrews.

John Ashton.

Ernest Spencer Cox.

Edward Wynne Donovan.

Edward John Erskine.

Robert Gilmour.

Herbert de Grave.

Arthur John Harries, M.D.

L. Pyke, F.C.S.

Thomas Knight Steanes.

Arthur David Stevenson.

Ernest Talbot.

Henry George Wood.

*Students :*

Geoffrey F. R. Barrett.

John F. M. Bennett.

William Elsdon Dew.

Henry Home.

Frank A. Jones.

Robert Andrew Miles.

Frank Wallis.

The meeting then adjourned.

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The Two Hundred and Fifteenth Ordinary General Meeting of the Institution was held at the Institution of Civil Engineers, 25, Great George Street, Westminster, on Thursday evening, January 22nd, 1891—Professor WILLIAM CROOKES, F.R.S., President, in the Chair.

The minutes of the Ordinary General Meeting held on January 15th, 1891, were read and approved.

The names of new candidates for election into the Institution were announced and ordered to be suspended.

Donations to the Library were announced as having been received since the last meeting from Mr. Wilfred S. Boulton, and Sir David Salomons, Bart., to whom the thanks of the meeting were duly accorded.

The following paper was then read :—

### ELECTRIC LIGHTING FROM CENTRAL STATIONS, WITH SPECIAL REFERENCE TO THE CHELSEA SYSTEM.

By Major-General C. E. WEBBER, C.B., (Ret.) R.E., Past-President.

In September, 1889, I read a paper before the British Association, at Newcastle, on the subject of the Distribution of Electricity in parts of Chelsea and Kensington, with accumulators as the main source of supply.

The Chelsea system was then described as follows :—

(a.) Charging the accumulators at high pressure, and discharging for distribution at low pressure, and effecting the changes necessary to the same by special automatic arrangements placed in each storage station.

(b.) Supplementing the main supply by means of continuous-current transformers.

(c.) Distributing by means of mains laid down in a network.

(d.) Maintaining a constant pressure by means of the intro-

duction into the discharge circuit from the storage stations of counter E.M.F. cells.

(c.) The use of underground conductors throughout, laid so that the cables can be drawn in and out, and renewed or repaired whenever necessary, without disturbing the ground.

For the benefit of those of my audience who have not already heard a description of the system, I will first refer to the arrangements and connections of the charging units, batteries, and transformer motors.

First, there are three of these plants in series charging one of the half batteries in each of three storage stations, the lighting being maintained by the other, or second half, battery; and the pressure in the distributing mains being regulated by cells which give a counter E.M.F.

Second, the three plants are charging the second half batteries. Those which having been previously charging, are now discharging to the distribution system. Regulation is still maintained as in the first position.

Third, there is a cessation of charging, and the two half batteries are discharging together in parallel. Regulation is maintained as before.

Fourth, continuous-current transformers, run by the same units, are supplying current to the system in parallel with both halves of the batteries.

The apparatus for effecting the various transpositions of the secondary batteries in the storage stations, and for maintaining the pressure constant in the mains, is automatic.

The movable portions are worked by the energy which would otherwise be wasted in the counter E.M.F. cells, which are used for regulating the pressure in the distribution system.

All the actuating portions of the apparatus require current at an E.M.F. of only 4 or 5 volts.

There are exhibited detailed diagrams showing the connections of the automatic regulator used in the storage stations.

For the use of these diagrams I am indebted to the kindness of the Electrical Construction Company, and their engineer in London, Mr. Frank King, who is the designer of the apparatus.

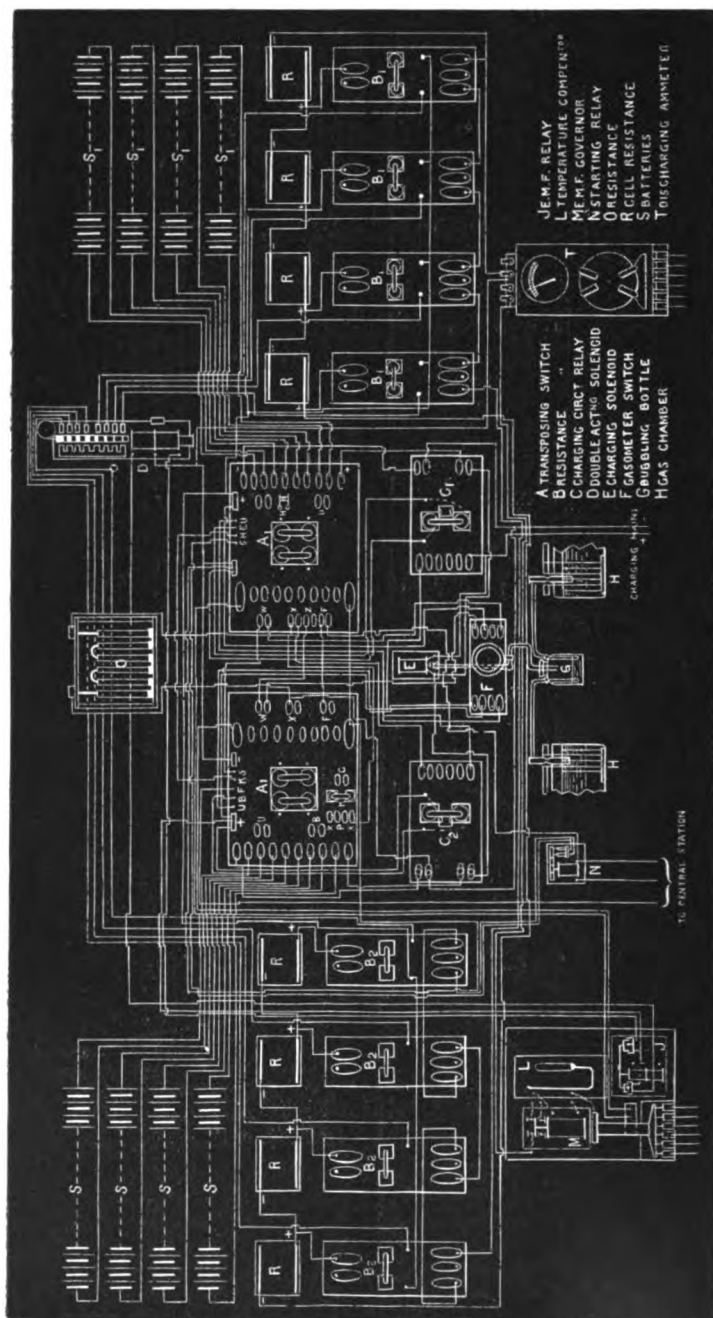


FIG. 1.—The Automatic Regulator.

The charging mains arriving from the generating station are divided, and connected to the groups of mercury cups of two "*charging circuit relays*."

These relays— $C_1$ ,  $C_2$ —are identical in construction, and are really rocking switches, and come into action at the moment when the half batteries are just in or out of charge.

Their function is to introduce into the charging circuit a carbon resistance,  $O$ , between the time that one half battery goes out and the other enters, the position of being "under charge."

The groups of four cups on the outsides of these relays are half positive and half negative, and the relative positions of the arms incline in the same sense.

To pull them into those positions and maintain them there, each arm is actuated from below by two electro-magnets placed in a local circuit, to which I will refer further on.

When the right half battery,  $S_1$ , is in charge, the right relay,  $C_1$ , is in contact on its right (as it presents itself on the diagram). The positive and negative cups are respectively in connection with corresponding cups on the outside of the right-hand "transposing switch,"  $A_1$ .

The intermediate terminals are severally connected with the poles of four sets, each of 55 cells, so that these 220 cells are in circuit in series.

On the way to the right-hand "charging circuit relay," the positive conductor has formed the coils of a "charging solenoid," marked  $E$ .

Its function is as follows:—When the current is passing, the solenoid closes a valve in connection with a little gasholder,  $F$ , used to collect the hydrogen gas formed by one plate in one of the cells under charge. This cell is called a "master cell." When no current traverses the coil of the solenoid, the valve is open, and no gas is retained in the holder.

At this point the duty of the holder,  $F$ , may be described. When the half battery is approaching full charge, gas is given off, and the valve alluded to being kept closed by the charging current, gas is retained by the holder, and commences to fill it.

The gas given off is collected first in an ebonite "gas chamber and acid trap," marked  $H$ . The trap prevents any gas from this particular plate going anywhere else than into the gas chamber.

From the chamber, the gas passes into a bubbling-bottle,  $G$ , with oil in it, which is intended to extract the moisture, and thence to the little holder.

The size of the holder is calculated so as to contain just sufficient gas as will be given off by one plate after the proper state of "milkyness" of the cells has been attained.

The moment it fills, a contact is made with the arm of a switch which is underneath it. This switch closes the local circuit to which I have already alluded as actuating the pulling-down magnets of the charging circuits,  $C_1$ ,  $C_2$ .

To return to  $A_1$ , the right-hand "transposing switch."

When its right-hand contacts are dipped in their cups and the left-hand contacts are elevated out of them, the charging current can go nowhere else but through the half battery.

Meantime, discharging into the lamp system is going on from the left-hand half battery, and while this is going on the right-hand contacts of the rocking arm of the left "transposing switch" are dipped.

These contacts are so arranged and connected with the poles of the four sets of cells that they are in position to discharge in parallel. It is evident that when the positions of the rocking arms of the two transposing switches,  $A_1$  and  $A_2$ , are

reversed, the left half battery will be "in charge," and the right half battery discharging; and, again, that if the inner ends of the two rocking arms are dipped and they stand with opposing angles, then both half batteries are discharging, and charging has ceased. In the perspective diagram below the right is discharging and the left is charging.

Let us return to what takes place at the moment the arms of the switches rock into a fresh position.

Underneath each of these arms, as in the case of those belonging to the charging circuit relays,  $C_1$ ,  $C_2$ , are placed two electro-magnets to pull them down. These electro-magnets are also placed in the local circuit mentioned twice before.

This operation of rocking over, so to speak, is sufficiently slow between the break and make in the mercury-cups as to allow an interval of disconnection of less than half a minute.

During this interval the charging current does not cease, but for the time, as has been already described, is relayed through the resistance  $O$  by the operation of the charging circuit relay  $C_1$ , the rocking arm of which, instead of moving slowly like that of the transposing switch, moves instantaneously.

The operation is effected in the following order and manner:—As has been stated, the gasholder closes the local circuit in the pull-magnets under the elevated end of the rocking arm of the right-hand transposing switch, which commences to move slowly over, aided by a balance weight rolling in a groove on its upper side.

As the charging contacts leave the mercury other contacts under the arm break the current in the pull-down magnet of the outer end of the rocking arm of the charging relay, which, being weighted, falls the other way, and effects simultaneously the introduction of resistance  $O$ .

The rocking arm of  $A_1$  continues moving slowly over. It first makes the contacts which bring the right half battery into the position of discharge in parallel, and finally makes other contacts, which, acting on the local circuit through a small rocking switch, start the rocking arm of the transposing switch  $A_2$ . Its first action is to cut the left half battery out of the lamp system, and when it completes its journey to throw the left half battery in series into the charging circuit. At the same moment, by a separate contact, it closes the local circuit which actuates the pull-down magnet of the charging circuit relay  $C_2$ , the effect of which is simultaneously to transfer the charging current from the resistance  $O$  to its next duty.

When the left half battery is charged, the next and final stage is carried out by the gasholder acting as before on the local circuit, the work of which is confined to inciting the rocking arm of the left-hand transposing switch to rock over, so that the second half battery goes to the assistance of its fellow.

A tell-tale wire back to the generating station from the transposing switch actuates a relay at the generating station the moment that the left-hand contacts lift out of the mercury. The current so transmitted actuates a relay which cuts off steam at the generating station.

On each side of the diagram are shown four "resistance" cells of the same type as those for accumulating, marked  $R$ , &c.

The duty required from them is as follows:—

1. To supply counter E.M.F. with which to regulate the pressure in the lamp system.
2. To give the required energy to actuate what has several times been referred to as the local circuit, of which there are actually two—one for each side of the apparatus.

This brings us to the consideration of what occurs in discharging from the batteries.

In a system requiring pressure of 100 volts, varying 4 per cent. above and below, that from 55 cells is excessive, even allowing for the fall between the station and the E.M.F. points in the system. At the same time it is necessary to have ample reserve to meet occasions of heavy discharge.

To keep the pressure constant at those points, there must be tell-tale wires from those points to the E.M.F. governor, M, fixed on the governing board.

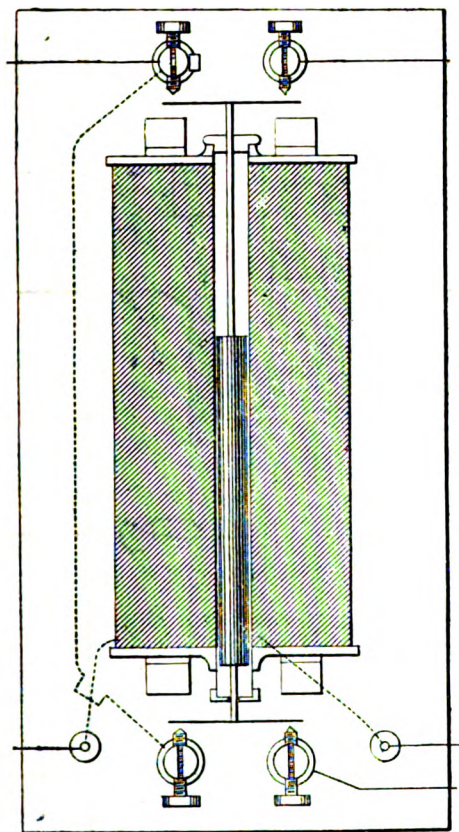


FIG. 2.—The E.M.F. Governor M.

This apparatus is a fine-wire coil in parallel with the feeders, and its function is, when the pressure is above or below the normal limit, to move a solenoid up and down. A lever bar connected with this completes a local in a relay marked J, which in its turn actuates a double-acting solenoid, D, which operates simultaneously on sliding contacts in connection with *resistance switches*,  $E_1 B_1 B_1 B_1$ , and  $B_1 B_1 B_1 B_1$ .

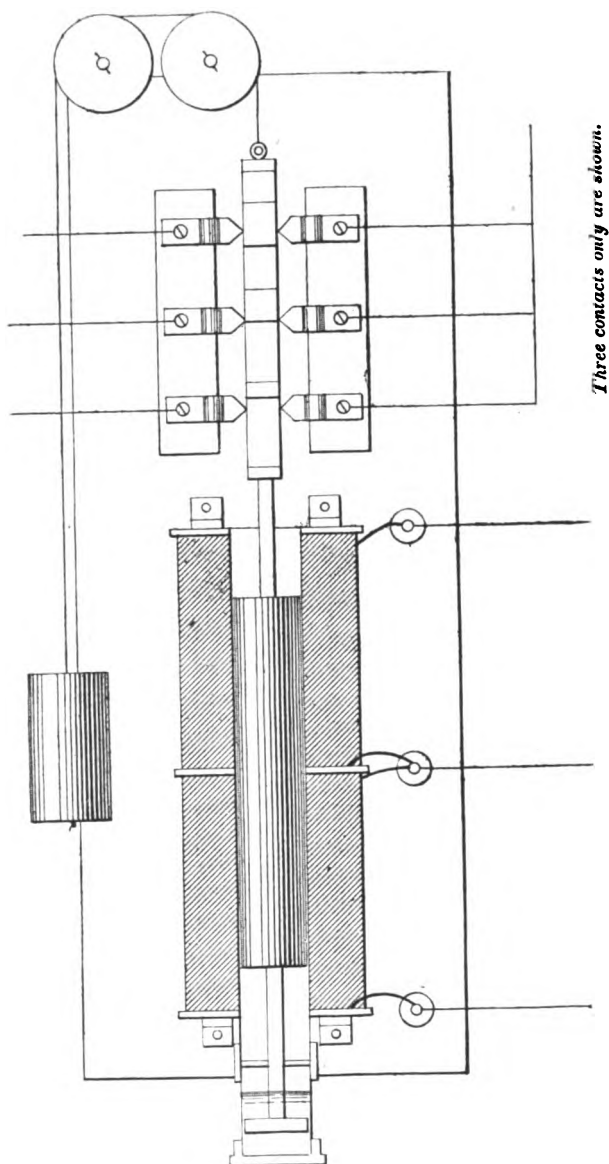


FIG. 3.—The Regulating Solenoid D.

This solenoid acts as a rheostat on the discharging circuit, by putting in or taking out the counter E.M.F. cells one at a time, and even two at a time.

The "E.M.F. governor," M, is also in series with a "temperature compensator," marked L. This little apparatus keeps constant the resistance of the

fine-wire coil of *M* by altering the length of a carbon filament which is in circuit with the coil of *M*.

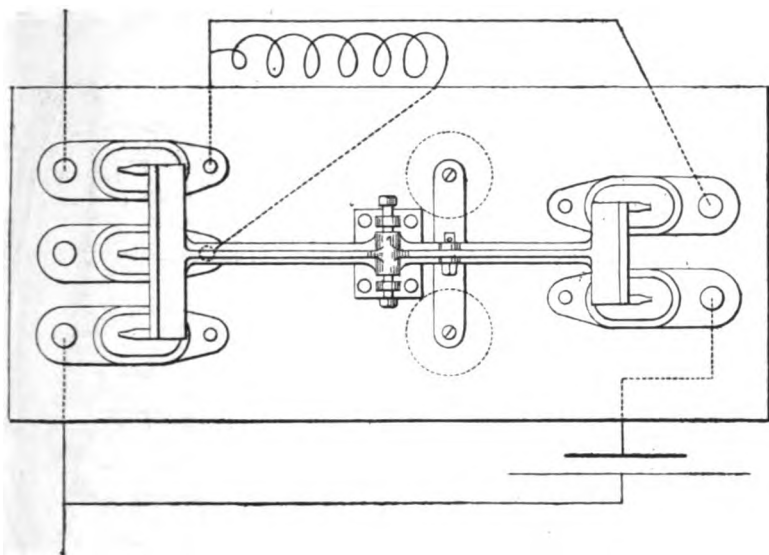


FIG. 4.—The Rocking Resistance Switch  $B_1$  and  $B_2$ .

This alteration of length, which minutely varies the resistance, is caused by the expansion and contraction of alcohol in a bent glass tube, which presses on a column of mercury, into which one end of the filament is inserted.

As the bulk of the alcohol alters through temperature the mercury is pressed up or down, and less or more of the filament is short-circuited.

The diagram shows at two places the connection to which the feeding mains of each kind are led.

At *T* is placed an ammeter to measure the total discharge into the feeding mains, which can be cut in or out. At the other pole is shown a set of safety fuses: these are actually used at both poles.

The small relay marked *N* completes the local circuit, and causes switch *A*, to be brought into the position of charge. It is actuated by a current from the generating station when the engineer is ready to start charging.

The adolescence of the system being safely reached, the Institution will be naturally desirous to know if the combination described in 1889 as "Accumulation of the main supply from a "generating station in storage stations, situated at suitable "distances from one another," is a success.

(a.) As regards the charging of the accumulators at high pressure, and discharging for distribution at low pressure, and effecting the changes necessary to the same by the special automatic arrangements placed in each storage station. The system

described above, and illustrated on the diagrams, has worked uninterruptedly in each of the four storage stations now in actual use.

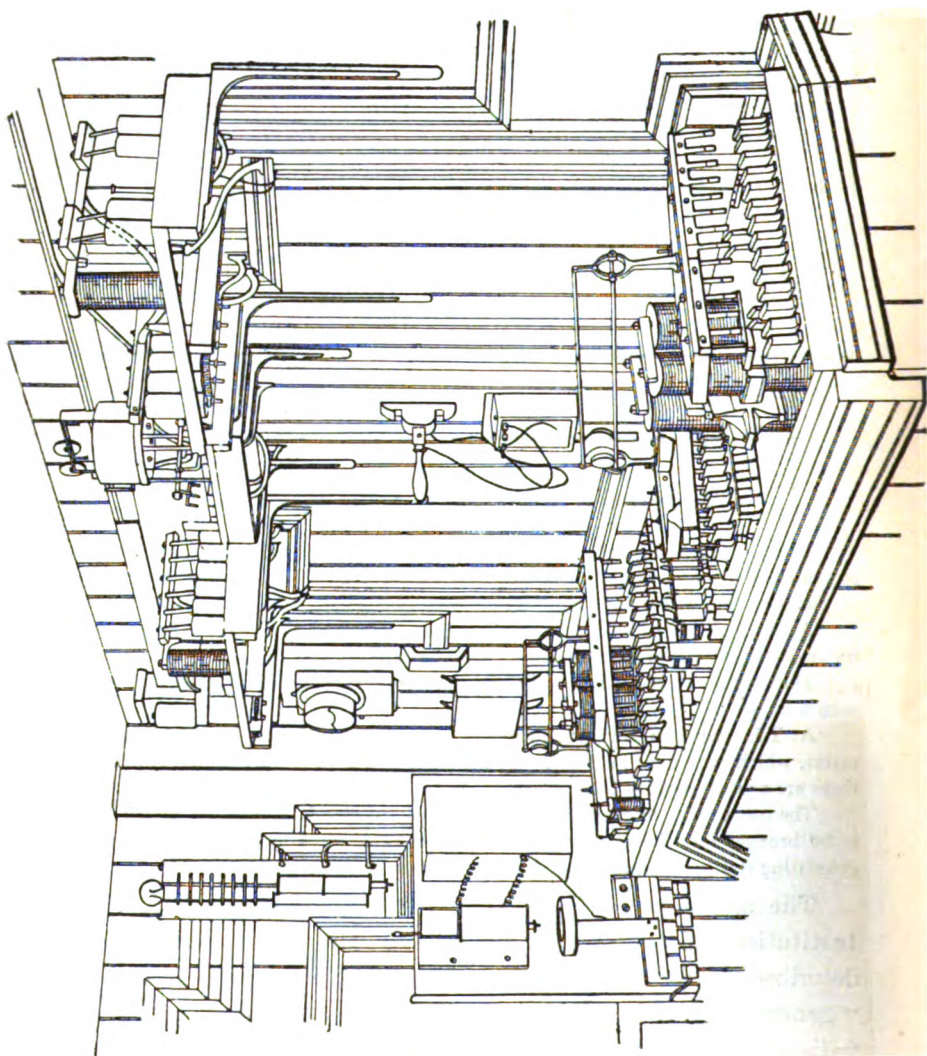


FIG. 5.—Perspective View of part of the Automatic Regulator.

The arrangement then described showing three units of generating plant in series, charging one of the half batteries in each of three storage stations, with a current of 75 amperes

and 1,500 volts in the charging mains, in combination with the automatic regulating plant by which the changes of the half batteries in and out of charge and discharge are made, have been in daily use.

The mercury cups and contacts have behaved perfectly, and there has been absolute freedom from heating in any of them.

The very small amount of alterations that have been since necessary is a proof of the care with which the Electric Power Storage Company made trial of the regulating apparatus before putting it to work for the Chelsea Company.

(b.) The supplementing of the main supply by means of continuous-current transformers is a portion of the system of which our year's experience has gone far enough to satisfy us that it is an efficient and reliable way of providing for the increase in demands for current which the full discharge of the distant storage stations would be incapable of meeting; and that the economy arising out of the employment of the generating units, during otherwise idle hours, compensates for the loss in conversion.

The first continuous-current transformer designed for the Electric Construction Corporation by Mr. F. King, and in use by the Chelsea Company, has been run on over 180 days for a total period of nearly 1,000 hours.

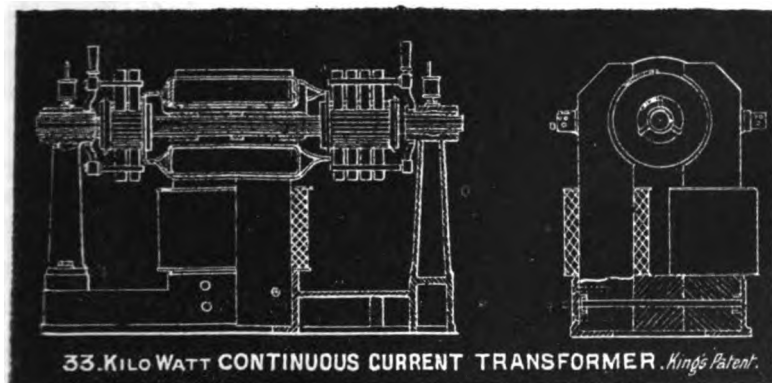


FIG. 6.

Careful tests, made last August in the presence of Mr.

Raworth, engineer of the Brush Electrical Engineering Company, gave the following results:—

		Volts.	PRIMARY.		Volts.	SECONDARY.				EFFICIENCY.. Per cent.
			Amps.	Watts.		Amps.	Watts.			
No. 1 test	...	588	64	37,632	110·5	280	30,940	...	...	82
„ 2 „	...	606	72·5	43,935	111·5	327	36,460	...	...	82·9
„ 3 „	...	604	72·5	43,790	111·5	320	35,630	...	...	81

The total weight of the machine (*vide* diagram) is 4 tons 7 cwt. 2 qrs.

The field-magnet armature and shaft are carried on a cast-iron bed and pedestal, having an over-all length of about 6 feet, and a height of about 3 feet 6 inches.

The armature is ring type, carried by a 3¼-inch shaft, having two commutators, one at each end. The primary circuit, or motor, commutator has 72 sections; that for the secondary generation has half that number, namely, 36.

The primary circuit on the armature consists of 72 segments, ·120 inch diameter copper, well-insulated wire, corresponding to the number of commutator sections, having a conductivity resistance, measured at opposite segments, of ·1755 ohm.

The conductor forming each section is wound in an ebonite channel laid between the copper strips which form the secondary, or generator, circuit, and arranged in a single on the outside and a double layer on the inside.

The secondary, or generator, circuit is composed of copper strip of ·0593 of an inch sectional area, in 36 coils, each of which is connected with a section of the right-hand commutator. The conductivity resistance is ·0087 of an ohm.

The core of the armature is composed of charcoal iron rings ·022 inch thick, insulated from each other, the external diameter of which is 17½ inches, and inside 8½ inches.

The field magnets are shown in vertical section in the diagram, the weight of the iron in them being 2 tons 11 cwt. 2 qrs. They are wound with ·095-inch copper, well-insulated wire, in 20 layers, having 2,720 turns on each limb, and have a conductivity resistance of 18·9 ohms, being connected in parallel with the discharging mains.

In Diagram (see Fig. 7), the machine is shown connected up

on the motor side with the conductors from a unit of generating plant, running with almost the same output as is required to charge one of the half-storage stations of the Chelsea system.

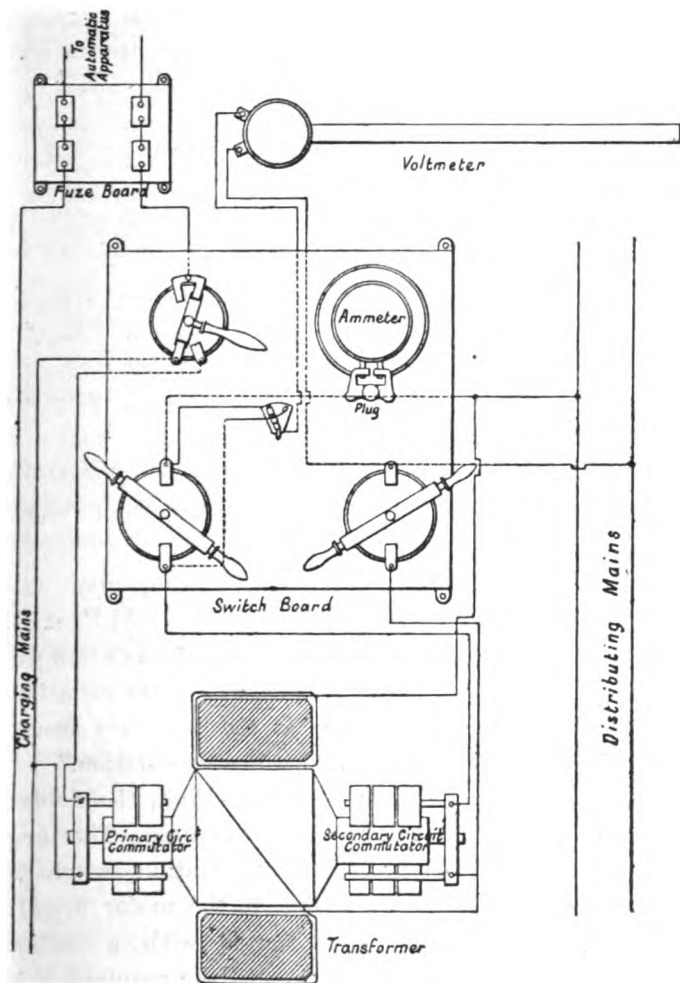


FIG. 7.

The collectors on the secondary circuit commutator are also shown on the diagram connected through switches with the low-pressure distribution systems, having measuring instruments of the output for pressure and quantity.

The behaviour of this machine, both as regards heating and

sparking, has been throughout all that can be desired, and as the type improves, we expect an improvement in efficiency.

Mr. King has favoured me with the following remarks :—

“The peculiar advantages of the combination of transformer and battery circuit which is at work at Chelsea, are the ability to, at all times, use the transformer to its fullest capacity, and therefore greatest efficiency both of engine, dynamo, and transformer; and the absolute regularity with which the potential at the secondary, or generator, terminals of the transformer will follow the varying potential at the junction with the feeders to the distributing main.

“It is, of course, easy to see the reason why the machine behaves in the manner indicated. Any fall in the potential of the discharging mains means a reduction in the intensity of the magnetic field in which the armature rotates, and with a constant motor current this reduction of magnetic field means an increase of speed, until the number of revolutions is sufficient to raise the E.M.F. in those mains so as to react on the fields.

“The action of the machine, therefore, is consequent on the intensity of the magnetic field, created by the E.M.F. existing at any moment on the discharge mains, and the strength of current in the primary circuit, which will cause variation in speed sufficient always to produce at the secondary armature an E.M.F. equal to that of the discharge at the station.”

The method of starting is as follows :—First, the fields are made by switching on the right-hand switch. The engine and dynamo are then connected at the station switch-board through the ordinary charging mains to the motor armature. The engine and dynamo are then started with a resistance in the field, and the speed increased until the required E.M.F. at the dynamo terminals is obtained. The potential at the right-hand, or secondary, terminals will, in consequence, run up to that in the discharge system at the stations. The left-hand switch is then closed, and the current increased in the primary circuit by the gradual removal of resistance at the charging dynamo, until the maximum capacity of the transformer is reached, which

will continue irrespective of the amount of storage discharge, upon which all variations of load in the system will then fall.

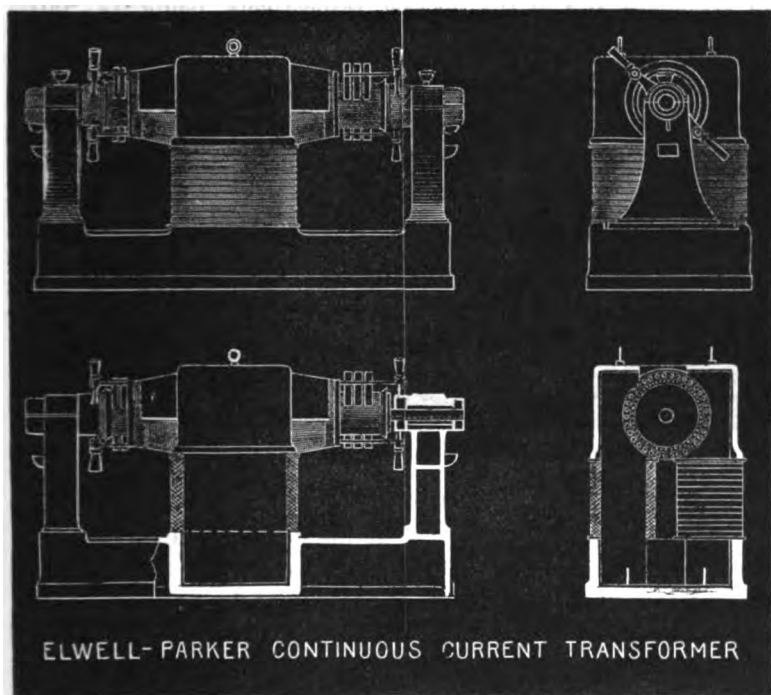


FIG. 8.

The tests of the manufacturers, which have not yet been verified by us with working loads and long continuous runs, are reported to show that—

- 1st. With an output of 16,000 watts, the efficiency is  $81\frac{1}{2}$  per cent.
- 2nd. With 20,000 watts, it reaches  $89\frac{1}{2}$  per cent.
- 3rd. At 40,000 watts, it attains 92 per cent.

The weight of the machine is 5 tons, and the floor space required for it is 7 feet by 3 feet.

The armature is of the drum type. The primary is wound with 200 turns connected up to a commutator with 100 bars. The secondary has 80 turns connected to 40 bars in the commutator at the opposite end. The diameter of the core is 14 and 15-16ths inches, and of the finished armature 16 inches. Its length is 22 inches. The winding of the primary is for 540 volts and

75 amperes, and of the secondary 108 volts and 350 amperes, with a speed of 550 revolutions; the resistances of the primary and secondary, and of the magnet, respectively, being  $\cdot 17$ ,  $\cdot 001$ , and 8 ohms.

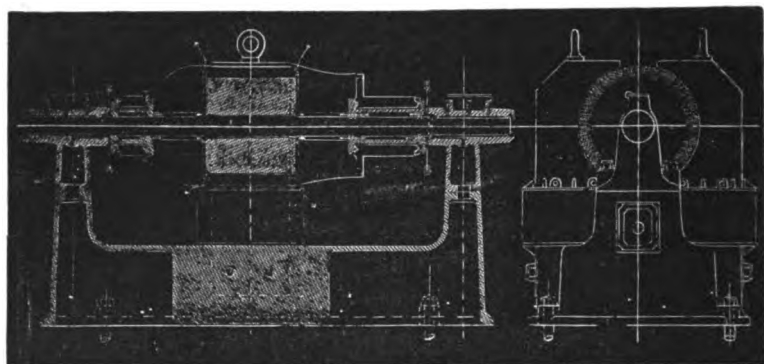


FIG. 9.—Laurance Scott & Co.'s 40-Unit C.C. Transformer.

The diagram shows the general construction of this machine.

The following description of it has been given me by the makers:—The conductors on the armature are laid in deep and narrow slots milled in the iron plates comprising the core; the low-tension conductor, which is of copper strip, being disposed in the bottom of the slots, with the high-tension, of fine stranded cable, above. There are 88 of these slots, each 1·8 inches deep and  $\cdot 22$  of an inch wide; one of the low and five of the high tension conductors being in each slot. The mechanical advantage of this arrangement is that it is impossible for the conductors to slip over one another to the smallest extent. There are 44 sections in each commutator, the low-tension having one turn per section, and the high-tension five turns.

Ventilation is secured by hollow spaces left between the shaft and each commutator, extending along the shaft to each end of the core, where  $\frac{1}{2}$ -inch spaces are left, extending to the outside of the armature. The paths of the air currents are shown in the diagram by the little arrows. The high and low tension circuits are insulated from one another, and from the armature core, by mica everywhere, except at the vertical ends; and, as an additional precaution against the one breaking into the other, caps of brass wire gauze, in electrical connection with the shaft, but carefully

insulated from the conductors, are arranged between the two windings at the ends, so that before the high could break into the low tension circuit at either end of the armature, it must come into contact with one of these caps, and therefore with the frame of the machine. In the core itself, the iron plates effect the same object as these caps, as they are purposely put in electrical contact with the shaft.

The weight of the whole machine is 65 cwt., and speed 660.

Current-density in high-tension winding with 75 amperes = 2,450 amperes per square inch.

Current-density in low-tension winding with 375 amperes = 2,080 amperes per square inch.

Loss in high-tension winding at full load, hot = 1,425 watts.

„ low „ „ „ „ = 945 „

„ field magnets ... .. = 402 „

Current in „ ... .. = 3.7 amperes.

Electrical efficiency ... .. = 93.7 %

Of the endurance or efficiency of this machine I am not as yet in a position to give results.

Of the efficiency of the service to the public in the Chelsea Company's district there has been ample evidence in the contents of a long list of testimonials from customers, which without exception are unanimous in praising the constancy, regularity, and perfect steadiness of the supply.

Questions have been publicly raised as to the maintenance of the pressure prescribed by the statute in the Chelsea district, otherwise I should not have alluded to it. (See Figs. 13 and 13A.)

Between the 13th and 19th of December last, the company found itself obliged, for the first time, to cut off the current from their system for short periods in the day, owing to the abnormal exhaustion of their reserve of storage, in order to save their accumulator plant from permanent injury.

Before doing so, the Board of Trade and the local authorities were communicated with, and the former was told that the step had been rendered unavoidable, not because of want of foresight, but through, it is only fair to say, the delinquency of contractors for machinery who had engaged to supply plant early in the

autumn. The company offered to lay before that Department all the documentary evidence necessary to prove that they had early in the year made ample provision for all conceivable emergencies, and that this late delivery of plant had been the sole cause of the unfortunate position to which they had been brought by the long continuance of foggy weather.

Owing to conditions which have nothing to do with the system, and of which a detailed and intelligible narrative would occupy too much time, the nascent period of five months, previous to the 1st October, 1889, has been held to be purely experimental.

During the 15 months ending with the 31st December, 1890, the growth of the demand has been as shown on the following diagram:—

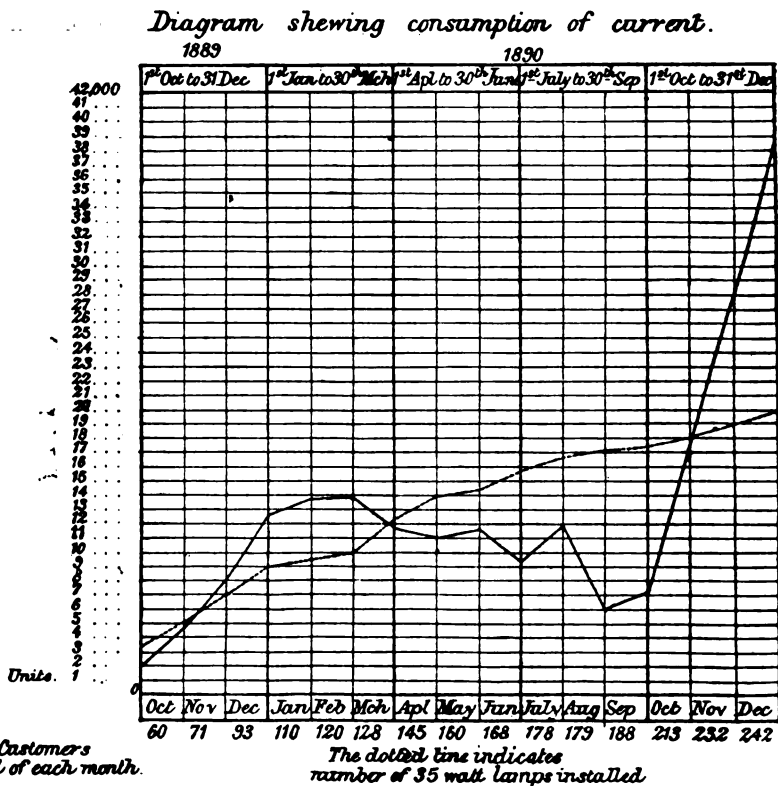


FIG 10

During part of the same period the following abnormal conditions, which preclude the possibility of an absolute comparison of results, have been in existence, *namely* :—

(1.) The details of the structure of the central station have been under completion and perfection; amongst other things, more than one kind of foundation for machinery in motion has been under trial.

(2.) The details of the machinery and accessories connected therewith (for instance, for condensation) have been in process of addition or alteration, to provide for extension.

(3.) As shown by the diagram, there has been a gradual growth of the demand, which, being at the commencement of the undertaking, is abnormal to the conditions existing in a matured system.

This growth has been accompanied by a variation in the demand according to the season, of which no forecast could easily be made, for the following reasons :—

(a.) The average consumption of gas in houses of the wealthier class varies so much, according to individual circumstances, that no comparison with it would be of assistance.

(b.) The nature and cost of other illuminants varies considerably, according to the tastes of the occupier of a house.

(4.) In very foggy weather householders use in the day as few gas lights, oil lamps, or candles as possible, but we find that on such occasions there seems some peculiar temptation to turn on every glow lamp in the house.

This variation in the anticipated demand has also been marked by its inequality in various parts of the same district. Thus, in one of the new residential gardens in the Chelsea district the custom on one side is rather above the average, whereas on the other side—consisting of 15 or 20 houses of an average rental of £400 a year, in front of which mains have been lying ready to supply for more than a year—as yet, in only one is current being supplied.

To provide for such contingencies as this in the first instance, would have entailed a first outlay in insulated copper, and a consequent idleness of capital, which prudence forbade. In some

cases the growth of demand at certain spots overtook the means of supply, with a temporary sacrifice of maintenance of the full mean statutory pressure.

The Chelsea system having started before most others, special experimental Board of Trade rules had to be summarily compiled, and these were more or less on their trial. The authorised rules, which dealt in an important way with the question of constant pressure, were only received in November last.

The best means of providing for uncertain special demands for current had to be prepared to suit the ordinary conditions of the system, as, owing to its comparative novelty, these could not have been designed before experience had been obtained of the nature of that excess.

Lastly, in first degree in importance, the Chelsea Company has, in common with other electric supply undertakers, had to make the best of the difficulty which has existed during the past year in obtaining delivery of material in face of the great demand on all electrical manufacturing firms, with a sensible sacrifice of economy in working.

As regards the efficiency of the generating plant, I will remind my hearers that the unit of power is one Willans & Robinson G.G. engine, geared direct to a 40 to 42 unit dynamo, which, running at its normal speed, can yield 70 to 75 amperes at 500 to 575 volts. This plant, when in full use, can be run charging a distant storage station from midnight to 3 p.m.; and from 4 p.m. to 11 p.m. it runs, by means of the same charging mains, a C.C. transformer (already described) placed at the same point.

In practice, the yield in the first period is—

38 units during 14 hours = 532 units.

In the second period—

44 units during 7 hours = 308 „

Total in the 24 hours = 840 „

Special tests of our Babcock & Wilcox boilers show an efficiency of 10.2 lbs. of water per lb. of combustible (ash and firing up being deducted); and that under such conditions it has

required 5·74 lbs. of combustible to produce each unit at the terminals of the dynamo.

This corresponds to 3·64 lbs. per I.H.P.—a figure which we are in process of reducing, and expect to reduce to 2·7 lbs.

The average loss in the charging mains, and by conversion with the two means in use, is just 28 per cent., and in the distribution between the converting stations, or sources of supply, and the consumers' terminals is 2 per cent. The 43 cwt. of coal used under the above conditions in the generating stations to produce 840 units delivers 588 units into the company's meters, or nearly 8·2 lbs. of combustible per unit of electricity sold, which, at 25s. a ton, is about 1·1d. per unit for fuel.

My qualification of this as a report of actual consumption is that, while it is accurate for periods of useful running of the best part of the plant, it is by no means so for those portions which it has not, for reasons I have given, been possible to run efficiently. Experience early showed us in what direction to work to obtain the maximum of efficiency originally claimed by Mr. King for the system, and that course has been, and is being, followed; but necessarily improvements in a plant which is constantly at work have to be effected with deliberation, and the rate at which they can be carried out more depends on the combination of the work of several manufacturers than on the supply company.

The accumulators in our storage stations have now been in use for periods varying from 21 months to nine months.

During these periods the work done each day by each station has varied considerably as between the stations and between the cells.

The routine of testing the 1,800 cells which are, exclusive of reserve, in constant use need not be described. Completely under control and supervision, it is, as can be understood, of a very perfect kind.

In one case, after 18 months' full use, it was found, from tests made by the manufacturers, that one-third of the active material of the positive plates had been exhausted. During the latter six months of the same period—which may be considered a good

example of normal rate of waste—the peroxide which has scaled off and fallen to the bottom of the glass cell and been removed has averaged half a pound per month from 15 plates, or half an ounce per plate.

The general conclusion come to is that under the normal circumstances of charge and discharge of the Chelsea system the life of the brown plates can be calculated at three years at least.\*

The possible leakage from accumulators has been laid some stress on by critics, but, with precautions adopted, our tests on an average show an insulation resistance to the earth of about 50,000 to 100,000 ohms.

In February, 1885, Professor George Forbes delivered certain Cantor Lectures before the Society of Arts. One of his objects was to assist electrical engineers in drawing out specifications, and selecting between systems. He then expressed his difficulty to be the probability that an expensive system proposed to-day would be superseded by a more economical one next year.

He referred to the low-pressure multiple-arc distribution as being “enormously expensive,” and the necessity with it of the generating station being in the centre of the district lighted, and also to the then absence of practical perfection of any system of high-pressure distribution.

Many of us can recall the able line of reasoning following which Professor Forbes was led to select 1·5 square inches section of copper to the maximum current of 1,000 amperes, and his exhortation to engineers to withstand the pressure of financiers, and to tell them honestly what is the most economical thing to do; and that if a financier wants to lay out capital in a way to cause a waste of energy, the engineer ought to have nothing to do with it.

I for one must always feel grateful to Professor Forbes for the valuable tables which he then compiled, and which have been

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\* I have been favoured by Mr. Hall, the electrical engineer of the P. and O. Company, with some very interesting facts connected with seven years' use of one set of accumulators, the details of which, with his permission, I propose to add to my paper.

of frequent assistance to me as a guide and check, even in the frequent cases in which I have been unable, for reasons of prime cost, to be guided by the results obtained from them.

We find that he then spoke of the choice of the district that it was proposed to light as being a matter requiring care, in order to give the best return for outlay of money and cost of working and maintenance. He compared the relative advantages of a district in which are many theatres, hotels, and restaurants, and one chiefly occupied with private houses. He deprecated any attempt to lay mains large enough to supplant gas entirely, and stated that it was better to trust to laying additional mains as required and as custom grew.

You will recollect that the text of the Professor's description of the system of distribution upon which he chiefly dwelt, and for which I am indebted to him for the loan of his diagram, was that "in the 'tree' system the amount of copper in all parts of "the mains is the same as if a separate wire came from every "lamp to the station."

You see, the diagram\* represents the plan of a city built in square blocks. With  $1\frac{1}{4}$  inches of copper to the 1,000 amperes, current is distributed to a distance of 124 yards from the generating station, which on the diagram is shown to be situated exactly at the centre of the city. To simplify his example, Professor Forbes supposed the sides of the blocks to be each 62 yards in length, so that the fall between the extremities of the diagonal—the line following two sides of the square—should not be more than 4 volts with a maximum consumption. As the length of the side of a block is 62 yards, and the distance dealt with is equal to the length of two of its sides, the distributing mains direct from the generating source are sufficient for eight blocks (see the portion of diagram coloured pink). Each of the four blocks next to the central station, with its two contiguous half blocks, has, therefore, its own "tree" of distributing conductors, their bases being at the station itself. The plant requisite for this area, being used under the same

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\* The diagram will be found in the *Journal of the Society of Arts* of October, 1885.

conditions, may therefore be the theoretical measure of the smallest unit of generation.

This gives a unit of supply to a rectangular area of 30,752 superficial yards, or about 175 yards the side, and each 100 8-candle-power lamps in use at one time within the area will require a pair of distributing cables, each cable having a section of about  $\cdot 05$  of copper, or  $7/13$ .

I do not know if Professor Forbes had in his mind any number of houses that should occupy the area of a square of 175 yards a side, or the number of lamps in use at one time in those houses. In a district like the north-east of the parish of Chelsea the average number of residences or shops within such an area is 93, and assuming that our experience is correct—namely, that each will use on an average a maximum of 28 8-candle-power, or 35-watt, lamps at one time—then the total number will be 2,500, and the current with a pressure of 100 volts required will be 880 amperes. It is safer, however, to allow 10 amperes per house, which would require 930, or a section of copper in all the distributing mains leaving the station of 2 inches, at 1,000 amperes to the inch. In like manner, each pair of stems which conduct the current to the base of a “tree” covering a similar area will together have a section of 2 inches, assuming that the generating units deliver current at a higher pressure between the terminals, for the more distant areas.

I trust the meeting will pardon my dwelling at some length on this diagram. It is very interesting, and helps the mind to realise several problems which must arise in the daily practice of many of my hearers. The first four “trees” already described are repeated at their outer margin by 12 “trees,” six of which are of the same form, and six having their base at the centre of the longest side of the triangle. Throughout, the maximum distance of 124 yards is adhered to, giving under the same conditions a fall of 4 volts at the most distant points.

Now, in order that the conditions of distribution in each of the triangular spaces occupied by a “tree” may be the same, it is necessary that the supply at the base points shall be the same in the second district as in the first, subject, as has been stated,

to exigencies of pressure. The 12 conductors from the source of generation to those points marked (2) on the diagram are called by Professor Forbes "*feeders*;" but also, and still more graphically, he calls them "*roots*." I may be also allowed to call them "*stems*."

When you look at the diagram, and recall the circumstances of the groups of streets and squares in towns with which you have to deal, you know well that in these old countries you do not often encounter anything like it. Your "*trees*" have been of every form and shape, and as often as not you have not been able to place the point of distribution, or, as we may call it, the "*base*" of the "*tree*," at a point in the block occupied by that "*tree*" which is nearest to the central station.

In like manner, there has been no uniformity whatever in a system in the question of distance of these points from the source of supply, nor has there been any rule by which the branches of the "*tree*" have tended to radiate uniformly from their stem. Like the gnarled and crooked stems and branches of the primeval forest, they twist and bend in every direction, and turn here and there, with startling indifference to the rules of symmetry.

These "*trees*," being of one nature and family, and having one kind of "*sap*," naturally unite at or near the extremities of their branches, and, lo and behold! a network is produced, in which the current from one "*stem*" is able to come to the assistance of the neighbouring "*tree*," when unnatural exhaustion in one direction has rendered increased alimentation necessary from another.

Claims to the exclusive right to use a network of electric light mains, fed at points with more or less frequent intervals, may shortly be made, and therefore I do not propose to follow this matter further, although there may be much to be said about it hereafter.

Whether we call them groups of "*trees*," or "*networks*," or their parts, "*stems*," "*roots*," "*feeders*," "*feeding points*," or "*bases*," we are all agreed with what Professor Forbes urged in February, 1885, *namely* :—

(1.) To use as large a number of points of alimentation and connection as we economically can.

(2.) That each of these points shall possess its own separate stem, root, or feeder.

(5.) The relative section of conductor at every point of a street to be the same as if every lamp had a separate wire going by the shortest street route to the central station.

But practice has shown us that we cannot, and need not, adhere always to two of his propositions, *namely* :—

(3.) That each distributing box must be nearer to the central station than any lamp fed from it.

(4.) That small groups of lamps should be fed from the distributing box by separate wires.

The distribution by means of a network of mains, in which sufficiency of supply is obtained with feeding mains reaching from the storage and transforming stations, or “sources of supply,” to points intermediate between them, is so well known and understood by the meeting, that except as an item in the combination known as “the Chelsea system,” I should not have alluded to it further.

A reference to it, however, necessitates a few remarks as to how it affects the question of the use of insulated or bare copper conductors.

As regards the economical aspect of the case, practice shows, in any comparison, that, where a small section of copper in each conductor in the same line only is required, other conditions being alike, it is much cheaper to use insulation of the character of bituminous compound, in ducts, or “ways,” of a small size and economical material, than to look for insulation to non-conducting supporting surfaces, with a large surrounding of air space subsisting in a subway of which the minimum first cost must be represented by a substantial figure.

It is in connection with large mains that the engineer who has first adopted the use of a dielectric material to surround his copper first turns his attention to bare copper, with a view to obtain a saving in construction. The size of these mains is controlled by the distance between the points of “source of supply” for a given demand in a given area.

Obviously, if these points can be placed nearer or further apart, according to what may be called the density of demand, there can be found a fairly definite limit to the size of the largest mains.

The positions of the feeding points in the areas between the points of "source of supply" are so much governed by conditions connected with the routes and communications of streets, that their theoretical frequency can rarely be maintained. In this respect only, then, apart from localisation of demand, need there be any uncertainty as to their size.

But where the "source of supply" is a large generating station, distributing at low pressure direct into a network of mains over a large area, say three-quarters of a mile a side, then I regard the adoption of a system of underground construction, such as that known as Mr. Crompton's, and used in part of Kensington, as unavoidable, owing to the necessity of using stems or feeders of large capacity. Indeed, such a system is the natural outcome of the conditions under which running machinery is the primary, and accumulation is the secondary, source of supply, with low-pressure distribution from one source.

On the other hand, when these conditions are reversed, the proximity of the "sources of supply" reduces the section of copper required for feeding the distributing network, and, as I have said, the cost of the underground works is by its simplification much reduced. Also, the position of the generating station is a matter of comparative indifference.

Accumulators used in connection with any system which virtually distributes direct from the dynamo at low pressure can be regarded only in the light of auxiliaries, and in no sense as a main source of supply. So far as I have observed, they act as little more than regulators, and are generally charged and discharged at the same time, and therefore not really fairly treated, and cannot be expected to have an average life.

The meeting will see that what is claimed for the Chelsea system in this respect is also possessed, under other circumstances of distribution, by any alternate-current high-pressure system, of which the transforming stations may be called sources of supply for low-pressure distribution.

I wish some one of my audience would, on an early occasion, favour us with a description of a well-designed converting station worked with transformers.

I am led at this stage to refer to the question of the candle-power of the lamp, to which supply companies refer when they give a popular estimate of the work they are doing. We hear of companies supplying 45,000 lamps, and others 100,000, and so on. Now the question is, What do they mean?

Not long ago a "glow lamp" meant a 16 or 17 candle-power lamp, consuming 60 watts per hour. If a calculation is based on such a unit, it is certainly not one that exaggerates the position of the suppliers.

Most engineers, if they thought in numbers of lamps at all, meant 10 to the nominal H.P.

Now I wish this meeting to understand that from the first I have been one of the few that have steadfastly opposed the 60-watt lamp being the standard, for several reasons. Some five years ago, at a lecture I gave to the Society of Arts on the subject of the manufacture and use of incandescent lamps, I gave practical illustrations of the effect on the eye of the direct and reflected rays of various sizes of lamps—I think they were 5, 10, and 17 candle-power—which showed that, so far as the appreciation of the eye is concerned, the naked filament only more and more paralysed the powers of vision, the larger the incandescent surface, and that the comparative effect on the sight of the reflected light of the various powers did not show proportionally to the disadvantage of the smaller ones.

In Chelsea we have from the first endeavoured, in the true interests, I believe, of an eventual large general consumption, to use the 8-candle lamp as our standard. In all circulars issued to the inhabitants and to customers we have consistently urged the use of that candle-power only, and the expediency, when more light is wanted at one point, of having rather two 8-candle-power than one 16-candle-power. We have pointed out over and over again that even in the largest houses, if there are no 16-candle-power lamps existent, their want is never felt, and that multipli-

cation of 8-candle-power at each point is far the best way of illuminating.

At the risk of having the appearance of an endeavour to multiply numbers, all reports and publications of the Chelsea Company since 1885 have spoken of 8-candle-power lamps as the standard; and I believe that by degrees this policy—the wisdom of which cannot, I submit, be doubted—is having its effect, and that grumblers at the cost of the light who began, in spite of our remonstrances, by using 16-candle-power lamps, are gradually adopting eights.

Any of my audience can have a practical example of the truth of what I say by visiting in succession Brooks's and the Devonshire Clubs, which are almost next door to one another. In Brooks's there are not half a dozen 16-candle-power lamps; in the Devonshire, I believe, there is not one 8-candle-power; and yet I defy any unskilled person to detect the difference without being led to observe it by being previously warned of its existence.

In Paris the Compagnie Popp, which now supplies current to a very large section of the city, means a consumption of 40 watts when counting its custom in lamps.

I understood from Lord Crawford's evidence before Major Marindin in April, 1889, when he spoke of the London Electric Supply Corporation being shortly about to supply many hundreds of thousands of lamps, 8 or 10 candle-power was meant.

I understand from Mr. Bailey's paper lately read before the Society of Arts that other companies are now beginning to mean the same thing when counting by lamps. I hope we may hear something on this point to-night.

It is from no wish to give an exaggerated idea of what the industry is doing that I press this point upon all. It is because that for all ordinary purposes, if a glow lamp is properly used, and placed so that the filament cannot be seen in all its naked dazzlement (so to speak) by the eye, 8 to 10 candles, or a 35-watt lamp, is amply sufficient at each point where light is required, under almost every circumstance of the needs of daily life.

Besides the example in the clubs which I have mentioned, I

can point to shops in which 8-candle-power lamps, well placed and distributed, have been more practically useful, and more satisfactory for business, than double the sum of their candle-power concentrated in a few "sunbeam" lamps.

While on this subject, I would remind the supply companies of the very hard facts that no assertions of theirs about their "custom" can refute, and these are, that candles and lamps in private houses, and gas in shops, are being proved to be much more serious rivals than they first pretended to allow. I think we may have our own selves to thank for some of this, because we have encouraged an unwise and wasteful application of our illuminant, and made it come out more costly than it need.

In the case of shops we have erred still more. We have overlooked the reasons for the successful rivalry of the Wenham and Sugg gas-burners, and, instead of showing how electric light can be used more easily than other means for illuminating the tradesmen's wares, we have followed the lead with a poor imitation of gas, which obscures the vision of the passer-by, and makes it impossible to examine the details of the articles exposed. I have studied this question in our own and foreign streets, and am convinced that, in the long run, electric lighting may suffer, for the reason that, whatever its advocates may now say, a large number of tradespeople who are now employing it, after a trial of its use and cost, are not likely to return to that of gas.

The other day, in Paris, I was asked to meet a wiring contractor on the subject of fitting up for electric light a handsome ground-floor show-room which has large plate-glass windows. He had no other idea than to suspend half a dozen arc or "sunbeam" lamps from the ceiling, each consuming, at Paris prices, about 6d. an hour, which would have had the effect, doubtless, of lighting the footway brilliantly, and of attracting some attention from the casual passer-by, but also of preventing anything inside the show-room being efficiently inspected by the half-blinded beholder.

No; I am sure that the electric light is winning its way slowly, but it is in spite of its misapplication, and of the injury to sight, to which it is slowly establishing an unfortunate claim. I am no

advocate for the other and equally unreasoning extreme of procedure by which light is wasted foolishly in frosting bulbs, or enclosing them in obscured and coloured globes. The subject is so simple that, were it not that one sees the grossest folly perpetrated in these directions, and again and again being repeated, it would be superfluous to waste your time over the subject.

One could value in hundreds of pounds the cost of the electric light that, on the one hand, is used injuriously to the eyes, and, on the other, is intercepted and absorbed just in the very path along which it should be allowed free way.

Another point which I feel should be understood amongst ourselves, so that we may all mean the same thing, is, that when a supply company talks of "supplying so many lamps," they should mean the total number of lamps installed with which they are in connection.

Very little experience shows—

(1.) That the proportion of the totals installed to the totals in maximum use at one time varies, notably between domestic and business, or public, use.

(2.) That that proportion is not constant all the year round in domestic use.

(3.) That the previous use of gas is no guide, except in shops and public buildings, in making an estimate of the probable number of glow lamps for which the current will be wanted.

The future and ultimate demand in any locality is one of the most difficult problems the engineer has to face.

In Chelsea we have come to the conclusion that out of five lamps installed in private houses two represent the maximum number in use at one time, and that we cannot expect that the gross earnings of each lamp installed will exceed 8s. 6d.; with the added fact that 70 is the average number of lights installed in private residences. This gives 28 in use at one time, at a cost of a little under £30 per annum at 8d. a unit.

I know that this compares unfavourably with such districts as St. James's; but Chelsea is an average residential district, and

St. James's is simply the cream of the world. I am sure that my friends who deal with the electric lighting of that part of London will understand that I am only drawing comparisons so as to give an idea of the demand in different kinds of districts. I see clearly that our experience heretofore in that direction shows us that the companies which supply current in what might be called the second-class districts have much to learn. We do not envy our brethren the fact that they have struck gold while we have only struck silver, and only ask them, in the interests of the industry, to make a good use of their luck.

In the districts in Chelsea and Kensington originally allotted to the Chelsea Company the total number of houses in the streets and squares within the areas of supply is 1,258. This number includes all shops and residences, except those in quite the smallest streets. In this area the ultimate possible demand might be for 86,000 8-candle-power glow lamps installed, two-fifths of which—34,400—would probably be the maximum in use at one time.

At present, of these houses, 252 take current, in which actually the equivalent of 20,000 8-candle-power lamps are installed, and of which the maximum of about 8,000 are in use at one time.

There are at present in the district four "sources of supply," and 17 intermediate points where a constant pressure is maintained.

The area within the parish of Chelsea which has been last added, and which contains no compulsory, or scheduled, streets, is circumstanced as follows:—The number of houses is approximately 4,400. The probable custom is likely for a long time to be confined to favoured spots, in which the ultimate demand will be in 230 houses, having 18,400 lamps installed. The ultimate possible demand in this latter area cannot be estimated until a large portion of it is rebuilt.

Experience is showing that occupiers who take the light do so either as a genuine luxury and convenience, for which it is well worth paying, or else to follow a fashion. After those have come in, they are followed by the "hesitators;" then there is a

lull; and, finally, the conditions which originally deterred the majority make them hold off for a very long time. After that, the accessions will only occur when changes of occupation, and other infrequent events, take place.

It would be to the advantage of the supply companies if electricity could be called the poor man's light. But when you consider that the cost to the company, together with that to the customer, of joining up a house, including the meter and safety apparatus, is, on an average, not less than £12, it is clear that even the £50 occupier is still a good way off from electric light for domestic purposes.

How much of the blame for this must we lay at the door of over-legislation, over-regulation, and over-speculation?

Assuming that all the houses in the first area—namely, 1,258—required the electric light, then allowing (which is accurate enough for our purpose) that each lamp installed uses  $12\frac{1}{2}$  Parliamentary units in the year, and that the amount consumed in such a district in the summer is to that in the winter quarters as 1 to 2, then each lamp requires in a winter quarter 8 units; and assuming that in these periods 3 units are required in each of the two winter months December and January, we get a normal consumption in a day of 24 hours that has least sunlight of 1 ampere-hour at 100 volts pressure.

In other words, the "sources of supply" in the districts named must eventually have a total capacity of 86,000 ampere-hours, when every house takes the current.

To meet an extraordinary demand on a foggy day (and, as you may have heard, Chelsea has lately had a rough time with fogs), I will double this, giving a maximum required capacity of 172,000 ampere-hours. Let us see if it is possible to meet this.

To do so, we must eventually have at each of six sources of supply a total discharging capacity from accumulators of 12,000 ampere-hours, with a maximum rate of discharge of 2,000 to 2,100 amperes; three transformer motors which discharge at a rate of 360 amperes, and at such times would run for 12 hours, giving 12,960 ampere-hours, or a total discharging capacity of 25,000 ampere-hours from each such station.

But there is a circumstance in connection with the Chelsea system that must not be lost sight of, and that is, the generating station stands inside the area of supply. Therefore conversion, either by means of accumulators or transformers, is not essential for distribution within a radius of, at any rate, 200 yards of the station.

However, conversion by accumulation would not be entirely abandoned as regards a portion of the demand on that "source of supply."

In the early days it has been thought well, while gaining experience, to convert here as at the more distant points; but at some future time the immediate surroundings will, to meet the condition of full demand, have to be entirely fed from low-tension continuous-current generators to the extent of a capacity of 25,000 ampere-hours.

My estimate thus gives a total output of 175,000 ampere-hours, but at present the above result is very remote indeed, for be it remembered, that we now have only 252 out of 1,258 houses on our books; that we originally estimated that one-third would be the limit for a long time to come; and that the provision which I have described above means that all may eventually come in; at the same time, I regard no system as sound which does not foresee the possibility of supplanting other illuminants in a majority of the houses, however remote that may be.

Whether there are signs of prudence, or imprudence, in these estimates, I await your judgment.

To meet one-third of that total possible demand the central station will shortly contain seven 75-H.P. units of power to charge the five distant sources of supply and drive one transformer motor at each, with three 105-H.P. units to generate directly at low tension and charge the storage reserve in the station when required to do so.

It is evident from the figures I have given, and the experience described, the power to meet the possible increased demand for some time to come, with 100 per cent. for fogs in reserve, will be secured.

Without going into financial details, it is easily seen how the

gross earning capabilities of the station and surrounding system of supply and distribution, when the above-mentioned stage of completion is reached, will exceed £13,000 a year when one-third the houses in the first area take current. How much of this may be expected to go to make up the net earnings is a matter I for the present may be allowed to forbear to suggest, as I am not at present engaged in compiling a company prospectus, but rather asking you to contemplate a sample of the industry under certain conditions from a scientific and commercial point of view. As regards the contention that a margin of capacity of 100 per cent. must be provided to meet the demand when bad fogs occur in midwinter, I submit Diagrams 11 and 11A, also 12 and 12A, for consideration.

In any system of distribution which employs—

- (1.) Charging mains between the source of generation and the source of supply ;
- (2.) Stems, or feeding mains, between the source of supply and the points where the branches of the “tree” diverge;
- (3.) Branches, or distributing mains, which start from these points of divergence and are connected with one another, or not, as the case may be;—

occasionally, in practice, instances are found in which the energy on its way to the consumers' lamps passes three times in one or other direction along the same route. Its first journey is caused by the need to convey it under comparatively high pressure to a point where it is to be converted to low pressure. The second is necessary in order that as uniform a pressure as possible may be obtained throughout a system. The third may arise out of the necessity to violate Professor Forbes's rule, and supply backwards towards the centre of generation from the end of a stem, where the branches diverge.

This, be it observed, may occur with any system of conversion, if the converting plant is concentrated in stations. Anything which avoids it constitutes at first sight one important and very fascinating factor in favour of using alternate-current transformers in parallel series, as is being commonly done now in the case

of many infant systems. The question is, must these systems be regarded as still in the stage "experimental," and must those who have laid them down be held to be wilfully shutting their eyes to their obligations under Provisional Orders—namely, to prepare to be able to supply every house—and are they only desirous of picking up custom casually where it is surest and largest?

Those undertakers who are now earning revenue with the service to theatres, hotels, clubs, and places of public resort, by such experimental means, would have still much to learn if, instead, they were dependent chiefly on domestic lighting.

The fourth condition of the combination which I have described as "the Chelsea system" is the means of maintaining approximate constancy of pressure throughout a network system of distribution, already described. It has, with slight variation of values, been maintained in full work at each of our storage stations since April, 1889.\* (See Fig. 13 and Fig. 13A.)

But however successful any means may be for providing a constant pressure either at the terminals of the sources of supply, or the far end of the mains forming (what I have ventured to call) the "stem," there is an obvious limit to the range which can be provided; but I may say that it has not yet been reached in our Chelsea system, either in the portions in which two conductors or three conductors are in use.

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\* "In October, 1889, Mr. George Prescott read a paper on 'Some Methods of Regulating Accumulators in Electric Lighting,' before the American Institute of Electrical Engineers. After giving a very interesting compilation of information, all published before, he goes on to describe a means of compensating for the rise and fall of the potential of a battery during the charge and discharge, and he alleges the existence of an unsatisfied requirement in this respect. The higher pressure from the charging dynamo demanded by the battery, he encounters (to save the lamp circuit) by the insertion of resistance, which he calls pressure-equalising; and his method of regulating the working potential of the battery consists in adding to, or subtracting from, the number of active cells in circuit. His so-called over-load and over-discharge switch is a mechanical arrangement with which to prevent the effects of irregular discharge, and his potential and current magnets to make the current-equalisers operate automatically. are but expedients in comparison with those I have referred to to-night, and which were in use here many months before the publication of his paper."









But it is not in this way that one practical difficulty is encountered. It is only necessary to remind my hearers that if two of such points (occasionally called E.M.F. points) have a distributing main connecting them, there must be, if the conductors are of uniform section throughout, a depression which diagrammatically will be represented by a curve of a form which will vary according to the distribution of the load.

To meet this condition of things, and help in economising copper, the Board of Trade, in the rules lately issued, prescribe that undertakers shall maintain a standard pressure which may be different for different portions of a distributing main, and shall declare to any consumer the constant pressure at which they propose to supply energy at his own terminals. Obviously it is directed that, once declared, that pressure shall not be altered or departed from without authority, except within prescribed limits.

These rules go further, however, and say that before using a distributing main the amount of such pressure (which, to meet an obvious condition, may be different for different portions of a main)—*i.e.*, the amount of those pressures—shall be determined, and given to the local authority.

Now, with great deference to the Board of Trade, our daily experience in Chelsea shows that this rule, made with the best intentions as a help, may be used to bear hardly on a supply company, if enforced to the letter. For instance, take a compulsory street, say with 30 houses along one frontage: we lay a main along it, calculating on a demand, say, from one-third of the houses, and an estimated maximum consumption in each. We don't know really how to proceed under the rule. At first, two houses, at irregular intervals, require current. What are we to declare to them and the local authority before letting them have it? Suppose we declare—what we know will be the case even if five or six houses require a supply—the same pressure throughout. Suddenly the owners of six more houses require current. This demand will at once cause a curve in the line of pressure; and, failing substitution of fresh figures in the declarations to some of the houses, near the centre of the street, which may be those that

first came on—a change of intention that would not be easily allowed—or a substitution of a larger conductor, a state of dilemma is produced which neutralises the beneficent effect of this rule, designed for an almost completed system, and not a growing one.

I ask my hearers if they do not think that some such result might have been anticipated by the authority which has framed those rules, adapted for a maturity which late experiences of the industry show is still in some cases very far off.

A wise foster-parent, without educational traditions as a guide, would not even pretend to expect that the period of training of youth shall be much more than experimental. But our foster-mother, the Board of Trade, has without waiting for experience laid down rules for the control of our youth, and put us under heavy legal penalties by Act of Parliament if even, without being wilfully wayward, we make one small mistake, through only an error in judgment, when attempting to meet totally unforeseen events.

It is revealing no secrets that when a committee of your Council was consulted, and gave much time in advising on and modelling these rules, great stress was laid by several of its practical members on the absolute necessity of all rules of the kind being at first no more than tentative.

This rule as to declaration is really no practical relief to the kind of difficulty which I have described; but there are circumstances under which it can and must be applied, and those circumstances arise out of the condition of things along the boundaries of the areas into which London has been empirically divided under the Electric Lighting Acts.

However much a company may desire not to avail itself of the apparent relief afforded by the permission to declare differing pressures, in practice they must do so at points at or near their frontiers.

Here we see the effects of Parliament trying to legislate without previous experience.

Which of you, even ten years ago, would of their own accord have violated every condition of balance of pressure, understood

by the engineer of every gas company, and laid down, that one network of electrical distribution should be conterminous with another, its neighbour, and yet be under a rigid law of "Thus far shalt thou go, and no further"?

Would you not have realised, without an effort of thought, that however dense the demand along the lines of that boundary, the service could never be so efficient as if the conductors were as mutually supporting as in the interior of the area?

A study of these boundaries in London gives a grotesque impression of the state of mind, as to electrical distribution, of the legislator who, burthened with that whimsical terror of so-called monopolies, first started with the idea of establishing an imaginary protection for the consumer by securing, as he thought, the power to the local authority to purchase the undertaking; the result being a geographical, instead of an electrical, boundary, determined by the comical shape of the London parishes.

It is curious to see how easily a coach and four has been driven through the aspirations of that legislator by at least one supply company, which, in securing powers to supply many parishes from a central station in a suburban one, has put the future acquisition of the undertakings by each local authority beyond the range of probability. And it is equally curious to find in other cases that two areas which should be mutually supporting are kept apart by the necessity of having to get an Act of Parliament before they can help one another.

(e.) The fifth part of the combination is the "use of underground conductors throughout, laid so that the cables can be drawn in and out, and renewed or repaired wherever necessary, without disturbing the ground."

This system, which the above description was intended to include, is the one known as the Callender-Webber system. Although an interested critic, I have not been lenient to it, and yet have no reason to go back on the reasons given a few years ago for resorting to it.

The network laid down in this system is situated along one or both sides of 41 streets, squares, and gardens of Chelsea and

Kensington. The total length, in yards, of line of route followed is 9,520—nearly  $5\frac{1}{2}$  miles.

The total length of insulated copper cable, varying between 61/12 and 19/15, in use for distribution of current is  $27\frac{1}{2}$  miles.

Besides the above, there are  $1\frac{1}{2}$  miles of pilot wires, making a total of conductors of 29 miles.

And the total weight of copper actually under the ground and in use is  $51\frac{1}{2}$  tons.

Without the power of withdrawing and renewing or replacing cables, we should in Chelsea already have had some labour and expense, having in several instances had to enlarge the mains. In this respect only—namely, that the conductors can be drawn in and out—is there any resemblance between this system and that referred to as Mr. Crompton's. But in the latter, to be able to do this, a box with a stone cover has to be placed over each point where the copper strip is supported on insulators.

The variety of sizes and shapes of the bitumen concrete cases being considerable, I can recall only one instance in which the minute space available between the crown of a vault and the underside of the paving of a footway has obliged us to resort to the use of iron pipes.

In my telegraph days I saw several proposals such as Mr. Crompton's, for stretching conductors in subways in a very similar manner. I may say that they all came from men who were not telegraph engineers. But Mr. Crompton has certainly pointed out a way of laying underground, and most successfully too, a system of copper conductors of unusually large capacity; and having carefully watched this work during its progress in Kensington, I cannot lose this opportunity of testifying to the excellent manner in which the work has been carried out.

But there are two limits to its applicability—one, where the probable demand is so scattered or sparse along any one route as not to justify so large an initial preparatory expenditure; the other, where the available space under the footways is insufficient.

The former limit is at once appreciable when it is understood that the conduit complete, *without copper*, costs at least 24s. a yard, providing for a fair engineering profit.

The latter will be best understood by those engineers who have had occasion to carry on much excavation under the footways of our streets. I say the "footways," because, owing to the frequency of boxes necessary to give access to the insulating supports, the system, though feasible, is not well suited for a line under the roadway.

In Kensington, Mr. Crompton has had the exceptional good fortune to find wide footways and small coal-cellars—not universal, but very general. In Chelsea, where new houses are numerous, they are scarce. If he had commenced work in the City of London, his system would have been impossible.

But even in Kensington, except by the long rows of high-class mansions, the system in its integrity has to be departed from very frequently. In the first place, every obstruction under the surface, at a depth of 18 inches, requires a diversion at right angles, and a second diversion to get into line again. At each of these points, which is called a "straining-up box," the cost is increased by something like £5 to £10. And taking an average of examples, these points occur at intervals of 100 feet.

In the second place, where space does not exist, or where the line has to drop suddenly to pass under the roadway, there, our old friend the tube and insulated cable has to be resorted to, and the whole system of insulation is altered. I believe I am under the mark in stating that these interruptions cover one-tenth of the average length in average streets.

And here, again, I have the experience of some friends, engineers in America, as to the arrangement of what might be called "layers" and "faggots" of wrought-iron tubes bedded in concrete, and of their use for a long time in American streets for electric light cables. They are men who, from the first, differed from Edison and some others in America and here, and who refused to stake their reputation on conductors buried and sealed down, with inaccessible insulation. But they all acknowledge that the 2-inch wrought-iron tube is a very costly "duct" in comparison with one made of bitumen concrete; and that an insulation made with vegetable gums, costing nearly double that with a bitumen compound, is out of place underground, when it can be easily drawn in and out for repair.

It is, in Kensington, to this costly alternative Mr. Crompton prefers to resort when his culvert system cannot get along. But I understand that bitumen casings and insulation are being used in Westminster and Belgravia.

I may be met to-day by reports that the system with which my name has been connected has not in every case, either in economy, or may be in efficiency, fulfilled all that is claimed for it.

In answer to such criticism, let me draw the attention of my audience to the fact that my "specification for the supply and "laying down underground of electrical mains" on the system used in Chelsea and part of Kensington was published a long time ago in the electrical papers; and all I can insist on is that any failure encountered has been due to a departure from the conditions prescribed therein.

As regards the comparative cost of the two systems, it may be of practical assistance to let the meeting have the following result of observation :—

Generally speaking, if the section of copper in a pair of conductors is 1 inch and under, then a main constructed with insulated cables in separate "ways" is cheapest—increasingly so with small conductors. Between 1 inch and  $1\frac{1}{4}$  inches the comparison varies according to circumstances. Over  $1\frac{1}{4}$  inches of section, then uninsulated copper in conduits bears away the prize for economy.

Actual experience in Chelsea enables me to give an example of the actual cost of a three-2-inch-way line laid in one of the squares.

Its length is 613 yards. The cost of supplying and laying the casing, including vestry charges and an allowance for engineering profit, was at the rate of 6s. 6d. a yard.

For purposes of comparison, the cost of the surface boxes must be added to the above. Allowing draw-boxes for large angle changes of direction, and one small service box for every two houses, we must add 2s. 3d. per yard, or a total of 8s. 9d. per yard.

On the three-wire system—for which provision is, I believe, always made in Mr. Crompton's mains—the three ways in the

above example can receive two bitumen high-insulation cables of 61/12, having an external diameter of 1·71 of an inch, and one 61/15, with an external diameter of 1·40 of an inch.

The section of copper in these three cables is 1·335 of an inch, and their cost per yard *in situ* is 31s. per yard, giving a total cost of the completed line at less than 39s. 9d. per yard, or if the section was 1·25 of an inch, about 39s.

This line is sufficient to convey current to all the 60 houses which are situated along the route.

If provision need be made for half the houses only, then the cables will cost 17s. a yard, or at a total of 24s. 9d. for the completed main per yard.

In actual practice 324 yards is an unusual length for a distributing main, and the ends of the stems being nearer to one another, the section of copper to supply 30 houses would be actually less.

Supposing that the space between the stones of the footway and the cellars of the houses did permit of the introduction throughout of culverts for the passage and insulation of bare copper—which, as it happens, they do not—even then the cost of the mains is not much less per yard if constructed on the bare copper system. For instance, the section of copper in the case first under consideration is  $1\frac{1}{4}$  inches, which in strip costs £53 per 100 yards, or 10s. 7d. per yard. To this must be added the cost of drawing in, which may be said to cost the same as for cable, but with the added cost of straining up.

In the case of the stranded copper, insulated, costing 28s. per yard, that cost may be divided equally between metal and insulation.

But to every 100 yards of the culvert system I have to add the cost of three straining points at, say, £7 10s. = £22 10s., and 10 yards of cables in iron tubes, which, with the additional labour, will add at least £5 to the cost per 100 yards, making the total come out not less than 29s. 6d. a yard without copper, or 40s. 1d. at least with copper. In neither case have I credited the usual discounts.

Recently, when in Paris, I was interested in having the

opportunity of watching the laying down of mains by the Compagnie Popp, and by the Compagnie Edison. The former uses a cast-iron trough about 8 inches wide and 6 inches deep, having a cover, and means of jointing one trough to the other, which are about 6 feet in length. These joints are by way of being water-tight.

The cables are laid in three or four layers, three or four in each layer, and are separated from one another by slips of unwrought deal, with a deal board resting on the slips between each layer. These cables are laid in dry while the line is being made. Mons. Popp assured me that, when required, they can be easily drawn out, and others drawn in in their place; but this remains to be proved.

The draw-boxes at the angles of streets, which have round iron covers in a square frame of the same material, are large, capacious inside, and consequently very convenient.

The service boxes are in front of each house, but owing to the great frontage of most French houses, they can be placed on the main at less frequent intervals than with us in London. In the case of shops, beside the service box with its T joints on the distributing mains, there is a second box, let into the shop front below the glazing, which can be opened from the outside, in which are placed the cut-outs.

The mains of the Edison Compagnie, which I saw being laid, consisted of exceptionally large cables of uninsulated copper strand. They were being laid in concrete troughs of about 15 inches in width and 10 inches in depth. They were supported, without any possibility of being strained, on porcelain insulators, and the troughs covered in with stone flagging. These cables not only approached one another, but also the sides of the trough so closely, that I was startled at seeing the covers being laid on and the earth and footway made good.

Being at a considerable depth—5 or 6 feet in many places—and having no access to the insulators, I cannot conceive how sufficient insulation can be attained or maintained. My impression was that the only obligation on the contractor was to get as many tons of copper under the ground as he could.

Elsewhere I saw the same company taking up insulated lead-covered cables out of the bare earth, which evidently had given way at intervals, or had proved too small. I watched this with the belief that the users had had their punishment, had suffered, and were at least benefitting by dearly bought experience, and that a rational plan was being adopted, and the buried cable immovable discarded. I felt that, even if not convinced of their error, the Municipality would refuse to allow the nuisance in the streets being perpetuated. But no; identically the same kind of conductors were being laid in the ground, and in identically the same way.

It would not do to let it be supposed that for purposes of small high-tension conductors I condemn all systems of construction which do not permit of the conductors being removed by being drawn out at permanent openings or draw-boxes, or which, in other words, place conductors underground in troughs and seal them in, so that access can be obtained to them only by disturbing the surface, excavating for, and taking up the line. I wish to retain an open mind as to what is best for the engineer to advocate under those conditions.

With low-tension distribution it seems evident that the size of the copper that may be eventually required cannot be easily estimated, and that, even if it could be so, it is not expedient to place underground quantities of copper which may have to lie partially idle for a long time to come. Hence the provision of sufficient capacity in culverts, ducts, or ways, into which copper can be introduced, either bare or insulated, as it is required, is a sound act of foresight.

But with high-tension distribution, accompanied by due caution in the execution of the work at the consumer's premises, the question can be regarded rather differently. For example, at Bath, where the "tree" system may be said to be in use, I have encountered the following conditions:—A stem of about 800 yards in length without any house connections on it; at the end of that two branches, traversing both sides in some cases, and one side in another, of two groups of

streets and squares: in one case five streets, containing 150 residences, are passed along; in the other group six streets, containing 92 houses. Suppose that each house requires an average of 10 amperes at 100 volts, it will require half an ampere at 2,000 volts. The distributing mains in the first case will have to carry 75, and in the other less than 46 amperes, or cables of an average of  $19/15$  and  $19/17$  respectively. Now it is obvious that the least proportion of the cost of these mains is in the conductors themselves, and thus some such system as that for which the Messrs. Callender contracted with Mr. Massingham at Bath is probably the most economical. Such a main is described in Messrs. Callender's specification for what they call their "solid system," and, exclusive of the restoration of the surface, of which the cost varies according to its nature and the scale of charges of the local authority, would cost, including engineer's profit and superintendence—of course, if executed in lengths of not less than 500 yards—less than 9s. a yard run.

The line, once down and well laid, need, so far as one can see, never be disturbed, except by accident or injury, and a sufficiency of capacity for probable future demands is secured without heavy cost. The main to which I refer has two insulated conductors of  $7/16$ , laid in cast-iron troughs 3 inches by 2 inches internal section, and having  $5/32$  of an inch thickness of metal, the cables are supported and kept apart by bridges of bituminous wood 2 feet apart, and the vacant space is run in solid with bitumen, including taking up the paving, excavating, laying, filling in, and restoring the surface.

In proportion to the number of cables and the section of copper, the cost is reduced in proportion, if four or more larger cables are laid. For instance, a main with a metal case containing four cables of  $19/18$  and four of  $7/16$ , and having together  $\cdot 23$  of an inch in section of copper, can be laid and completed for about 15s. a yard.

I have had some of these lines tested for insulation resistance. In the case of one pair of  $19/18$  cables, with only two service branches (on which the transformers were disconnected for the time) in a length of 1,450 yards, which had been buried over six

months, I found an actual resistance of over 100 megohms, or  $82\ \Omega$  per mile. With the primaries of the two transformers in circuit, the result under the same conditions was 15 megohms, or  $12\cdot4\ \Omega$  per mile.

Another pair of similar cables, 1,500 yards in length, with seven transformers in circuit, gave only 3 megohms, or  $2\cdot5\ \Omega$  per mile.

As regards the question of depreciation, and cost of renewals and maintenance, we have a good deal still to learn. There are clearly four main classes the conditions of which affect those prices, as well as that of capital cost, *namely* :—

(a.) Bare copper in the case of which the conductors, and their means of insulation, are inaccessible without excavation.

(b.) Bare copper where insulators are accessible, and the conductors can be drawn in and out without special excavation.

(c.) Insulated conductors which can be drawn in and out without excavation.

(d.) Insulated conductors which are fixed, and which, except at the boxes, are inaccessible throughout their length without excavating.

I may say that I have not yet seen a case of (b) class in its integrity—that is to say, without having in combination short lengths of (c).

Only lately the London Chamber of Commerce has rather startled us by issuing a circular in which it lays down the rule that depreciation for mains with “bare conductors insulated by “means of glass, porcelain, or other such like imperishable “materials,” may be calculated at 1 per cent.; while that on “all “cables or other conductors covered with continuous insulation” should be calculated at 5 per cent.

Now, if that body had accompanied their statement by a few words of explanation as to whether the calculation is to be on the cost only of copper insulated in the one way or the other, we might have thought it worth while to discuss with them the question, even though it is quite outside their province to lay down any such law. But as we are left to suppose that the cost

of the whole structure of the main may be included in the figure from which the percentages are to be deducted, I really do not think it worth while to take up the time of the meeting beyond drawing marked attention to the document, which probably several present have seen, in the hope that some speaker, struck with the adventurous nature of the statement, as I have been, may be led to give an opinion on it in the discussion which I hope may follow.

There are several points on which, if time permitted, I might have given some interesting information, especially on the experience of 18 months with the Aron meters, also the results of the use of recording volt and ammeters. For many of the details of my subject I am indebted to the members of the staff of the Chelsea Company, to whose unremitting labours and attention, under novel and difficult circumstances, most of our success is due.

The PRESIDENT: This is a very important paper, bristling with facts, and one which must be of the greatest interest to most of those present. Many of you, I am quite sure, will be glad to take part in the discussion to which it must give rise, but which at this hour could only be very short, and I think it better, therefore, to defer it to our next meeting, on February 12th. In the meanwhile, I beg to propose that a hearty vote of thanks be accorded to Major-General Webber for his very valuable paper.

The vote having been carried by acclamation,

A ballot for new members took place, at which the following candidates were elected :—

*Foreign Member :*

Robert Winthrop Blackwell, A.M., LL.B.

*Members :*

W. Stepney Rawson, M.A.      |      S. Ziani de Ferranti.

*Associates :*

Henry Charles Atkins.  
Fitzgerald V. Dalton.  
Edward de Facien, sen.  
Edmund R. Dymond.  
Samuel Edgar Fedden.  
H. C. K. Fisher.  
Percy Gye.  
Fred. H. Hadfield.  
Francis Harman Lewis.  
A. Hill.  
James Colin Lockley.

Walter Markby.  
J. A. Peggs.  
Frederick Ryan.  
Alfred Louis Sax.  
Charles William Sax.  
John Vaughan Sherrin.  
W. S. Smith, B.Sc.  
William Snelgrove.  
William Humphris Winny.  
George Charles Turner.  
Reginald Wood.

*Students :*

Arthur Brooksbank.  
William Carver.  
Walter Claypoole.  
A. Percy Donnison.  
Walter B. Lock.  
Frederic Charles McQuown.

Arthur Pettman Patey.  
Frederick George Shrewsbury.  
Charles Edward William Talbot.  
Percy Mason Williamson.

The meeting then adjourned.

## ORIGINAL COMMUNICATION.

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### EARLY HISTORY OF THE TELEGRAPH IN INDIA.

By P. V. LUKE, C.I.E., Member.

In tracing the history of the electric telegraph, it will be found that India was not behindhand in the investigation and employment of the novel method of communication which scientific discovery placed at the disposal of mankind. Although the first faint glimmer of the possibility of communicating by means of electricity was apparent nearly a hundred years previously, it was not until 1837, the first year of Her Majesty's reign, that a really practical electric telegraph was patented by Messrs. Cooke and Wheatstone, and set working for public use between Paddington and West Drayton. In May, 1839, Dr. O'Shaughnessy, an Assistant Surgeon and Professor of Chemistry in the Medical College at Calcutta, induced by what he read in periodicals regarding the attempts made in Europe and America, instituted a series of experiments in communicating telegraph signals by means of electricity.

To carry out his experiments, Dr. O'Shaughnessy constructed in the Botanical Gardens a line of iron wire, No. 14 B.W.G., supported on bamboos. To economise space and have the whole length under control, the bamboos were placed in 42 rows, separated by intervals of 6 feet, and the wire was carried backwards and forwards, 1 foot apart, until there was half a mile of wire, carefully coated with tar-varnish, on each row, making a circuit of 22 miles of wire in all. A tent was pitched in front of the entire line, and the connections so established that it could be tested from centre to flank to ascertain the effects of wire varying in length from 1 to 11 miles at either side.

This was the first telegraph line constructed in the East, and over it the following experiments were tried:—The effect of the electric current as measured by (1) the sustaining force of an electro-magnet; (2) the deflection of the needle of a galvanoscope; (3) the decomposition of iodide of potassium; (4) the

action of an induction coil. The result of the experiments was that, although communication was found to be more or less practicable by all the methods, Dr. O'Shaughnessy came to the conclusion that the last plan gave the best promise of success; and he proceeded to work out a novel and interesting system for corresponding by means of the electric shocks experienced by a person holding in his hands the ends of the secondary wire of an induction coil whenever the circuit of a battery attached to the primary wire was made and broken.

As a mode of communication by this plan of imparting shocks, he arranged a pair of watches or clocks, the dials of which were replaced by dials having the letters of the alphabet inscribed on them in three concentric circles, with one alphabet on each circle, so that a seconds hand in each revolution pointed to the same letter three times. The hand took three seconds to pass each compartment, of which there were 20, a complete revolution being made in one minute. The two clocks, one at each end of the line, were set going at the same time, so that the hand of each pointed to the corresponding letter at the same moment. The observer, holding the ends of the wire, watched the hand travelling over the dial, and when he felt a shock, noted the letter indicated, having first received, by one, two, or three shocks, intimation of which circle he was to look at. This most ingenious system depended, of course, upon the exact synchronism of the clocks at either end, which at that time must have been very difficult, if not impossible, to maintain. Dr. O'Shaughnessy no doubt found this to be the case, as further attempts in this direction were abandoned. Several highly successful modern systems of telegraphy, based upon this principle of synchronism between the instruments at either end of a line, were, however, foreshadowed by these trials at this early date in the history of the telegraph. Afterwards, when Dr. O'Shaughnessy came to work a telegraph practically, he adopted the second method, namely, that of communicating signals by the deflection of the needle of a galvanoscope.

Some months previous to these experiments Dr. O'Shaughnessy discovered, from the accidental falling of a wire into a large tank

at the Medical College, that when water was available only one insulated wire was requisite for completing communications. The discovery of the "earth return" had at this date already been made in Europe by Steinheils, but the investigation of it in India appears to have been an independent one—undertaken by Dr. O'Shaughnessy before he was aware that the fact had already attracted attention.

Fully convinced by these early experiments of the practicability of an electric telegraph, Dr. O'Shaughnessy next endeavoured to convey his conviction to the minds of the authorities composing the Government of Bengal and the Court of Directors of the Honourable East India Company. He appears to have encountered all the opposition and delay usually met with by pioneers of science, for the next ten years were occupied with innumerable experiments and endless reports. He persevered, however, and in 1851 the requisite permission was obtained, and the beginning of a system of telegraphs was made by the erection of a wire along the banks of the river Hughli to signal the shipping. The lines were from Calcutta to Diamond Harbour, with a branch from Bishtopore to Moyapore, and an extension from Kookrahutty, on the further side of the Hughli, to Kedgeri; making, with some short additional sections, a total of 82 miles. In 1851 four offices—at Calcutta, Moyapore, Bishtopore, and Diamond Harbour—were open for actual business, and two others—at Kedgeri and Kookrahutty—were added in February, 1852.

The lines first constructed were partly overhead and partly underground. The underground line consisted of an iron rod,  $\frac{3}{8}$  in. in diameter, weighing 1,939 lbs. per mile, made up from separate lengths, 13 ft. 6 in. each, all welded together end to end. This rod was then covered with two layers of Madras cloth, saturated with pitch and tar, spirally applied, the layers overlapping each other, in opposite directions. The rod was buried in a trench 2 ft. deep, by the edge of the road. It was placed within a row of roofing tiles, half filled with a melted mixture of sand and rosin, and as it was laid the tiles were completely filled up with the same mixture. When cold the mass was as

solid as iron. The trench was filled in with wet clay and well rammed. It may be noted here that some of this underground line was dug up in the vicinity of Calcutta in 1888, and that not only the iron, but even the Madras cloth, was found to be in a perfect state of preservation after 37 years of interment.

The overhead line was for the most part the same  $\frac{3}{4}$  in. iron rod, supported on bamboos, 200 to the mile, with sometimes eight or ten sal, teak, or ironwood posts per mile, in addition, to give strength. Dr. O'Shaughnessy was evidently very proud of the line he had constructed, for in a report he says: "The over-ground lines differ totally from those in use in America, England, or any other country, in the following most important point:—No WIRE is used. Instead of wire, I employ a thick iron rod,  $\frac{3}{4}$  in. diameter, weighing 1 ton to the mile, while elsewhere the heaviest wire is 1 cwt. to the mile."

For this heavy rod he claimed the following advantages:—(1) Immunity from damage by storms; (2) if thrown down, it would still remain uninjured; (3) it could not be broken or bent maliciously without the use of tools; (4) the conductivity was so great that no insulation was required; (5) no straining up in tension was necessary; (6) it admitted of rusting to an extent fatal to wire; (7) it was not liable to injury by birds or monkeys. Needless to say, he very soon discovered that a large extension of the telegraph was impossible with such heavy material, and that experience soon led him to modify his views. He always, however, retained his belief in wire of a heavy gauge, and to this day there are thousands of miles in circuit in India weighing half a ton a mile. Many of the advantages he claimed for a thick wire are undeniable, and of special importance in certain districts even at the present time; while it is worthy of note that this, the first actual telegraph line he ever erected, worked and kept up communication for five years—that is, until it was reconstructed in 1856.

One of the very first difficulties to be encountered in the early days of telegraph enterprise in India was the crossing of rivers, and in 1849 experiments were being made in this direction. A massive iron rod laid across the river Huldee, uninsulated, was

first tried, with a signalling station on each bank. Signals were passed, but it was found that careful and skilful operators were required, and that interruptions to communication were very frequent. Dr. O'Shaughnessy next tried working across a river without any metallic conductor at all, "using the water alone as "the sole vehicle of the electric impulses;" but though he succeeded in transmitting signals, the battery power required was enormous, and too expensive for practical purposes. Small as was the measure of success attained at that date, it was highly creditable, seeing that the problem of working across rivers without an insulated conductor is still exercising the minds of telegraph engineers. The subject of working without an insulated conductor seems to have had great interest for Dr. O'Shaughnessy, for in 1858 he made numerous experiments in the lake at Ootacamund, and in his report for that year he says: "I have "long since ascertained that two naked uncoated wires, kept a "moderate distance—say 50 or 100 yards—apart, will transmit "electric currents to considerable distances sufficiently powerful "for signalling with needle instruments." He adds that he had in the previous month (September, 1858) worked to a length of more than two miles on the needle.

Several attempts to insulate a wire were made at this time—one by coating it with wax and tape, and another by enclosing it in a split rattan, and paying the rattan round with tarred yarn—but it was not until copper wire insulated with gutta-percha was obtained from England that signalling across a river was made really practicable. As a mechanical protection for the insulated conductor many means were tried. One method was to enclose it in parallel guards of iron wire or rod, fastened at intervals by transverse bands, or loops, of iron; another was to secure it in the angles of a chain cable. The cables in every case were constructed, with great ingenuity and much labour, on the banks of the river they had to cross. The telegraph cable across the river Hughli evidently required special precautions against dragging anchors, for it was further protected by signal guns fired when ships approached, as well as by beacons, guard boats, and notices by the Marine Department.

The receiving instrument employed on this, his first, system of telegraphs was a small horizontal galvanoscope, designed by Dr. O'Shaughnessy, and worked in conjunction with a reversing key and a battery of platinum wire and zinc plates. An alarm was formed by a similar instrument in connection with a "Sam Slick" clock, which it caused to ring, and every instrument was protected by a lightning-discharger. All the instruments then in use were made in India, and the efficiency of the design and workmanship is testified to by the fact that it was not until 1856 that others of an improved pattern began to be obtained from England to replace them.

In these early days the charges for telegrams were as follows:—To any station from Calcutta to Diamond Harbour, four annas (sixpence) for each word of two syllables, and one anna (three halfpence) for each additional syllable; to any station beyond the Hughli on the Kedgeri line, eight annas (one shilling) for each word of two syllables, and two annas (threepence) for each additional syllable.

The utility and value of rapid communication between Calcutta and the sea was fortunately emphasised at this critical time in the history of the telegraph by the hostilities which broke out in 1852 between India and Burma. At a time of public excitement the services of the telegraph were brought prominently to notice, and its capabilities were tested with complete success. The steam frigate "Rattler," bringing intelligence of the first operations of the war, had not passed the flagstaff of Kedgeri on the 19th of April, when the news of the storming and capture of Rangoon was placed in the hands of the Governor-General in Calcutta, and posted on the gates of the Telegraph Office for the information of the public.

In April, 1852, Lord Dalhousie, as Governor of Bengal, finally convinced of the utility of the new method of communication, laid before the Government of India a Minute in which he proposed the construction of telegraph lines all over India, from Calcutta to Bombay, Agra, Peshawur, and Madras; and, further, the deputation of Dr. O'Shaughnessy to England to give evidence before the Court of Directors, and to assist in the despatch to

India of the requisite telegraph material. Dr. O'Shaughnessy's unremitting exertions for the previous 12 years in the interest of the telegraph in India now met with their reward. He went to England in May, 1852. The Court of Directors sanctioned the proposals of the Governor-General, and immediate steps were taken to give effect to them.

By November of that year he had inspected the home and foreign telegraph lines, and ascertained the condition of telegraphy in Europe. Sixty enlisted artificers were placed in training at Warley; contracts were issued for all the stores required; a manual for the guidance of the persons to be employed on the works in India was prepared; and in October, 1853, the task of constructing a network of telegraph lines all over India was taken in hand. By March, 1856—at which date Dr. O'Shaughnessy was again deputed to England on duty—great progress had been made, and telegraph lines, constructed in a little over two years, extended from Saugor Island Lighthouse to Peshawur; from Agra to Bombay; from Bombay to Madras, Mysore, and Ootacamund—3,756 miles of line, with 55 offices. On the 1st of February, 1855, the telegraph in India became an Imperial system open for the use of the general public, the short lines previously described being merely a local system connecting places on the Hughli between Calcutta and the sea. The business done during the first complete year was represented by 51,533 private and 9,008 State messages, making a total of 60,541, valued at 310,390 rupees.

The rapidity with which the first trunk lines of telegraph in India were constructed is remarkable, and was rendered possible only by the assistance given, under orders from the Government, by every Department of the State. Wooden poles were collected, and all the required material distributed as far as possible before the "Warley artificers" were set to work upon the actual construction of the lines. The supports first used for the wire, which weighed 1,200 lbs. per mile, were most varied. In the parts of the country where timber was abundant they were of teak, sal, or ironwood. In other parts slabs of granite and sandstone, and masonry pillars, were employed. The wooden

poles were frequently set in screw-piles of cast iron. Dr. O'Shaughnessy says of the line from the Tungabudra River to Madras, with a branch from Bangalore to Ootacamund: "This is beyond doubt the finest line in the world. The total length is 662 miles. Of these, 322 miles are erected on superb granite obelisks, 16 feet high above ground, in single slabs, and 174 miles on stone masonry pillars, capped with granite. Of the remaining portion, 161½ miles are on good teak posts, with screw-piles, caps, brackets, and insulators; and 4½ miles are underground."

It was very soon found that white ants destroyed the wooden poles. In 1856 Dr. O'Shaughnessy advocated the use of a short cast-iron post fitting into the screw-pile socket, carrying a light wooden spar for the support of the wire, and thenceforth a gradual change from wood to iron marked the selection of supports for the telegraph wire. The earliest lines were all made with wooden posts, bamboos being thought very highly of, and palm trees, fir, sal, teak, and ironwood being gradually brought in. When it was found that white ants spared nothing, a screw-pile socket of cast iron was introduced on the lines constructed between 1853 and 1856, and the use of a cast-iron piece 6 feet long in connection with it was advocated. In 1857-58 wrought-iron tubular posts came to be used: first, what were called "half" standards—the lower half of iron, the upper of wood; then "three-quarter" standards, in which the wooden portion was reduced to one-quarter; and finally "whole" standards—all iron, such as generally used at the present day. In 1857 the screw-pile sockets were upon occasions put to a use for which they were not intended, by the mutineers, who drilled vent-holes in them and used them as cannon.

In the early days, prior to 1857, Dr. O'Shaughnessy entertained some curious ideas on the subject of telegraph construction. One of his ideas was to have a number of different routes to a given place, *in order to avoid the objectionable practice of placing several lines of wires on the same posts*. With a single line he said that experience gained on 7,000 miles in operation showed that insulation could be altogether dispensed with, even during

the heaviest rain. In a report somewhat later, he says that "from numerous and careful experiments during last rains, it has been ascertained that electrical insulation is of very small necessity or importance, even when many lines are on the same posts. After the first shower has washed off dirt and dust, subsequent rain causes little or no leakage from wire to wire." He soon, however, changed his mind about this, and was one of the first to patent an insulator—a vulcanite cup fitted into an iron hood with sulphur cement, known as the "Brooke" insulator. This insulator outlived its day, for in his report for 1876 the then Director-General says: "I attribute the bad working of the earlier Indian lines to the Department being inflicted with the Brooke bracket and insulator, both of which are most thoroughly unfit for the purpose for which they were designed." From the time of Dr. O'Shaughnessy up to a very recent date each head of the Telegraph Department has marked his era by the invention of an insulator.

The earliest attempts to insulate an overhead wire were made by wrapping it for 2 feet with a triple coating of cloth, pitch, and tar, and placing it in the groove of a dry wood bracket. The bracket was then set on the top of the post in a nick filled with sand and rosin cement. Over the whole was placed an earthen pitcher painted with hot pitch and tar, to throw the water off. Another method was to use dry wooden brackets on which were placed the necks of broken bottles. The first imported insulator consisted of an iron cup filled with sulphur cement, and for quite a long time sulphur was adopted in various forms as the insulating material. In 1860 the Director-General (who was knighted 1856) says: "From experiments made on mountain lines on very long spans of wire, it is proposed to use very lofty iron masts in the plains, erected at long intervals. Numerous advantages will be gained by this plan, specially as regards facility for the insulation and erection of multiple wires, and immunity from most of the ordinary causes of interruption."

In the matter of telegraph instruments the circuits were at first worked with the small needle instrument, before described as being in use on the lines between Calcutta and the sea. Sir

William O'Shaughnessy admits that when he was at home in 1852 he became aware that the Morse instrument was the best in every respect; but the clamour of inventors was such that, before deciding on any particular pattern, it was necessary to send out to India for trial duplicate sets of every instrument then in use or proposed. Experiments made in 1854-55 clearly established the superiority of the Morse instrument, and in March, 1856, he received orders to go to England to arrange for its introduction. On his arrival, he found that the Morse system had been generally adopted in Europe, and he at once ordered a supply of instruments from Messrs. Siemens & Halske, of Berlin. This same year a class of young gentlemen—first of 40, subsequently increased to 74—was formed at Gresham House, to be trained and sent out to India to introduce the Morse system. In 1857 the first detachments of what were called "Morse assistants" went to Calcutta, Bombay, and Madras, and the change from the needle to the Morse system was gradually inaugurated.

From March, 1856, to December, 1857, Sir William was absent from India, and during his absence the Telegraph Department was superintended by, first, Lieutenant Chauncey, and afterwards by Captain Stewart, R.E.—the "Pat" Stewart of Mutiny fame. From March, 1856, up to May, 1857, the progress of the telegraph was satisfactory in every respect: 980 miles of new line were erected, including 300 miles along the railway embankment from Burdwan to Patna, constructed by Mr. Adley, engineer to the East Indian Railway Company, and taken over by the Government of India. Then came the Mutiny, by which progress was terribly retarded. The detailed accounts of the events which happened at this memorable time are of the deepest interest, but only some of the chief incidents can be mentioned here. The first section of line destroyed by the mutineers was that between Meerut and Delhi; subsequently the whole of the lines, Agra to Indore, Agra to Cawnpore, Agra to Delhi, were totally demolished—the posts being used for firewood, and the wire cut up for slugs. The lines also between Allahabad and Cawnpore, and Cawnpore and Lucknow, were wrecked. Great

was the extent of ruin wrought on the telegraph lines, and great also was the glory with which the Department covered itself in the gallant behaviour of its staff. Mr. Charles Todd, the assistant in charge at Delhi, fell in the general massacre, but not until his office had signalled to the Punjab the terrible events at Meerut, and the march of the mutineers on Delhi. The value of that last service of the Delhi office is best described in the words of the Judicial Commissioner, Mr. Montgomery: "THE ELECTRIC TELEGRAPH HAS SAVED INDIA." That message led to the prompt disarming of the Native regiments in Lahore and Peshawur; and as the line from Delhi to the Punjab was kept open during the whole time of the siege of Delhi, the telegraph rendered inestimable service to the Government of India and to the whole Empire.

As soon as the ascendancy of the British was re-established, the restoration of the telegraph lines in a temporary manner was accomplished with extraordinary rapidity and determination. To show the spirit that prevailed amongst the officers and staff, the following is quoted from a superintendent's report:—"Mr. Todhunter worked up from Indore, opening offices at Beowra, Bursud, Ragooghur, and Goona—this while officially warned to quit the line, both by Sir Robert Hamilton and myself; his answer was: 'I am well mounted and well armed, and I will hold the stations to the last.' He kept his word. Signaller Higgins, at Beowra, buried his instruments before he left. The office was soon in flames, but the instruments were uninjured; and when General Mitchell, on the 15th September, attacked and dispersed Tantia Topee's forces at Beowra, Mr. Higgins dug up his instruments and re-opened communication the same day." One of the most interesting occurrences in the story of the restoration of the lines is the dashing exploit of Captain Stewart, Mr. Harrington, Mr. McIntyre, and Mr. Devere, who ran up a flying line from Cawnpore to Lucknow with the final advance of the Commander-in-Chief on that city. It was generally admitted that the operations before Lucknow were greatly aided by the electric telegraph, and the cool intrepidity and ready resources displayed on that occasion gained the hearty applause of the whole army.

It was not only by the destruction of its lines that the Mutiny disastrously affected the Telegraph Department. Many of its best men were murdered, while others left to join in the stirring duties of a military life. Moreover, the *morale* of the Department seems to have been greatly upset, for Sir William says, on his return from England in December, he found a state of confusion existing that almost baffled all efforts to restore the discipline that once existed. Under his personal influence, however, the telegraph speedily recovered from the effects of the shock it received. The old lines that had been destroyed were reconstructed, and new ones were erected, until by the 31st March, 1858, they extended to a total length of 10,000 miles in operation or under construction.

It was in 1857 that a start was made with the telegraph in Ceylon. The construction of the lines was entrusted to officers of the Indian Telegraph Department, and by the end of the year they extended from Galle to Colombo and Kandy; thence northward to Manaar, whence a cable across the Gulf of Manaar to the island of Pamban connected the Ceylon with the Indian system. The laying of this cable—25 miles in length—was in those days an operation of considerable magnitude and importance, and Sir William refers to “the masterly feat Mr. Wickham has performed in placing the telegraph cable across the Gulf of Manaar with a native sailing vessel, and during bad weather.” He goes on to say: “The operation was as difficult, the line as long, the navigation at least as dangerous, as that of placing the cable across the Straits of Dover, for which a squadron of steamers and costly machinery were employed. Mr. Wickham performed his task under sail, and with no other apparatus than the rude windlass of a native vessel.” The cable was of gutta-percha, protected with iron wire guards, and was part of 100 miles obtained from Messrs. Glass & Co. and Messrs. Henley & Co., and sent out from England. It continued in working order up to 1867, when it became defective, and was replaced by one with Hooper’s core, which, after being several times repaired, is working at the present day.

Owing to faults from various causes, the subterranean lines

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that were tried in India were not a success. In 1855-56 a length of 15 miles of underground line was laid between Calcutta and Barrackpore. It consisted of a gutta-percha core, lead-covered, and laid in a trench without further protection, and failed within a very short time of its completion. A similar fate attended experimental lines of gutta-percha core encased in lead laid between Attock and Peshawur; in Calcutta, Bombay, Madras, Bangalore, and other places. Underground lines were at this date given a fair trial, and were only relinquished with reluctance when found to be unsuccessful. The failures of lead-encased core were probably due to the softening of the percha by heat, or mechanical injury, for in some specimens of lead-covered gutta-percha core dug up in Bombay in 1882, the percha was found to be in very good condition after being down for 29 years. Dr. O'Shaughnessy in 1859 published some "Instructions for the Construction of Subterranean Lines on a New System," which he prefaced by stating that the failure of subterranean lines was mainly due to the absorption of oxygen by the percha when exposed to the air or buried in dry earth, which converted it into a resinous substance. He writes: "This fact was ascertained by myself, and reported to Government in 1856. The elaborate experiments made by Dr. Hoffman, at my request, the year before last, in the laboratory of the Royal College of Chemistry, in London, fully corroborate the statements made in my report. The change in question does not take place under water; it is retarded, but not prevented, by coating the gutta-percha with tarred tape or yarn. It is prevented altogether by coating the gutta-percha with lead; but experience in India has shown that the lead-covered article is especially liable to injury by softening of the gutta-percha by heat, and the copper wire consequently sinking through the softened coating and coming in contact with the leaden tube."

In 1858 the system of receiving by sound was introduced, and a school established at which probationers were trained in reading telegrams by ear. It is curious to note how opinions have changed as to the value of this method. In all countries except America and India there appears to have existed a pre-

judice against receiving by sound, and it is only comparatively recently that the "sounder" has been introduced in England to any great extent. In India the first to recognise the superiority of this method was Mr. (now Sir Albert) Cappel, and early in 1858 he proposed its adoption. His suggestion seems to have met with small favour at first, as the following telegram to him from Sir William O'Shaughnessy goes to show:—"Receiving by ear is, in my opinion, almost as objectionable as by the eye with the needle, and defeats the real object with which the Morse has been introduced in this country." The superiority and convenience of the system, however, was such that it soon asserted itself, and in 1860 Sir William says: "The saving effected by discontinuing the use of the tape amounts to at least 30,000 rupees per annum, while twofold greater accuracy is obtained in our work."

On the reorganisation of the Indian telegraphs after the Mutiny, many improvements were introduced, and among these was the "safety coil" for protecting instruments from lightning, which was first adopted in 1858. It consisted of a coil of 20 yards of very fine wire, and was used in addition to a "plate" lightning-arrester, its use being attended with marked success.

In another direction, too, the year after the Mutiny witnessed an important change. The original tariff for telegrams was fixed at one rupee for 16 words per 400 miles of wire, with an extra tax of eight annas for each additional group of eight words. Fifty per cent. was charged for repetition if the sender wished to ensure accuracy—a very necessary precaution in those days. In 1858-59 a reduction was made by charging according to distance as measured on the map instead of by wire distance; and an anna for each word above 16 per 400 miles was substituted for the "group" charge. These reductions, Sir William O'Shaughnessy said, "rendered the Indian telegraph tariff, in a very large degree, the cheapest in any country." At this day it is still probably the cheapest in any country; for where else can a telegram be sent, to be delivered the following morning, a distance of 3,000 miles for eight annas, or about ninepence? The year 1858-59 was also marked by the introduction of "telegraph stamps,"

which were brought in with the view of enabling persons residing at places where no telegraph station existed to send their messages prepaid to the nearest telegraph office.

So far, the history of the telegraph in India has been the biography of the man. In June, 1860, Sir William O'Shaughnessy left India, never to return. At that time there were 11,000 miles of lines, and 150 offices working. The total number of messages sent during the preceding year in all India, Pegu, and Ceylon was 202,428. The revenue of the Telegraph Department derived from private (*i.e.*, excluding State) messages was 423,991 rupees, and the expenditure 1,720,427 rupees. In his last report to Government—that for 1858-59—Sir William concludes by predicting a great future for the telegraph in India. “By perseverance and determination it should be “made,” he says, “the best in the world, inasmuch as it “possesses a unity of organisation unattainable elsewhere, with “all the resources of the Empire to promote its extension and “improvement. In two or, at most, three years from this time “the lines should yield a clear profit, and a uniform minimum “charge for messages may then be adopted for all India. This, “with the general use of some simple cypher by habitual corre- “spondents, will enable the telegraph to perform much of the “present business of the Post Office. Meanwhile, we have at our “disposal, at a moderate cost, an instrument of such miraculous “power that by a single message it has already saved our Indian “Empire; while day by day and hour by hour it is busy in the “promotion of commerce, and the furtherance of private interests “of every kind.” After an extended tour over all parts of India, he adds, he seldom met a family who had not some anecdote to tell of the services the telegraph had done them. “There are “few Europeans in India who have not experienced a thrill of “pleasure when they meet our masts and wires on the margin of “every road, and know that these true tokens of science and “civilisation and power traverse our Indian Empire to its utter- “most limits. Should I see them no more, I can truly say that “I shall ever continue to take the most heartfelt interest in the “prosperity and improvement of the Department, and feel proud

“and happy that it has been my lot to bring it even to its “present imperfect state.” Without dealing with the Department as it exists at the present day, it may here be said that the prediction has been amply fulfilled; and it is a matter of satisfaction to know that the man to whom the Department owes so much lived long enough, although retired from public life, to witness results following upon his early labours that are perhaps without counterpart in the British Empire.

Sir William O'Shaughnessy, the father of the telegraph in India, died at Southsea on the 8th of January, 1889. He was the first man in any part of the world to construct and work a long line of telegraph, and to his indomitable energy and perseverance India owes the establishment of its system of telegraphy at a period which placed her at least on an equal footing with the most advanced countries of Europe. He devoted his life to the accomplishment of a grand task, in which he nobly succeeded; but as for fame, it is probable that his name is known but to very few, even in India, outside the Department over which he presided with such marked ability and such unqualified success. No statue marks the appreciation by a grateful people of the benefits accruing from his labours; nothing perpetuates his memory save a small portrait, presented by his eldest daughter, which hangs in the signal room of the Telegraph Office in Calcutta.

Up to 1860 the telegraph in Burmah—or Pegu, as it was then called—was not connected with the Indian system, but in that year the line was made which completed the communication. It went from Dacca to Prome, and included three cables, one of which—that connecting the islands of Borong and Ramree—was 64 miles in length. This cable was very lightly armoured, and failed within two years. After an interval of another two years, during which there was no connection between the Indian and Burmah lines, through communication was reopened by the completion of an overland line between Akyab and Taunghup.

From the time of the retirement of Sir William O'Shaughnessy up to 1865, Major (afterwards Colonel) Douglas held charge

of the Telegraph Department. His era was marked by the extension of the lines to 13,390 miles, the number of offices to 172, and the revenue to 10 lakhs of rupees. He devised a modified form of Morse sounder and key, which were manufactured in India, and proved highly successful. Numerous instruments of his design, and bearing his name, are still in use. He also introduced several new patterns of insulators. Up to his day the only insulators employed were the "Brooke," before mentioned, and a bell-shaped stoneware one, neither of which were satisfactory. Colonel Douglas's idea was a double-cup porcelain insulator covered with a massive iron hood. In those days it was considered necessary to protect the porcelain from mechanical injury, chiefly from stone-throwing. Education and civilisation generally seem to have changed matters in this respect, for protective hoods have long been abandoned, and the insulators do not suffer at the hands of mischievous boys any more than in other countries. In 1860 the 24-hour system was adopted for timing telegrams, and the use of a.m. and p.m. discontinued. Although considerable progress was made in the development of telegraphy in India at this period, it is clear that the guiding hand of the "father of the telegraph" was much missed; for it is on record that from 1860 to 1864 complaints from the Chambers of Commerce, the public newspapers, and other quarters, as to the hopeless inefficiency of the Department, were very frequent.

Colonel Robinson, R.E., succeeded Colonel Douglas as Director-General in 1865, and commenced his reign by the entire reorganisation of the administrative staff, the introduction of a new tariff, and the compulsory use of stamps (up to that time only optional) in lieu of money payments for telegrams; besides inaugurating much-needed reforms to improve the signalling staff and reduce the number of errors in messages.

So far, the superior staff of the Department had consisted of the 60 Warley artificers and 74 Morse assistants recruited and trained in England during Sir William O'Shaughnessy's incumbency, supplemented by certain others selected by competition and sent out from England under covenant, and a few men

engaged in India during the time of Colonel Douglas. In 1868 arrangements were made for regularly recruiting the staff with well-educated young gentlemen, nominated by the Secretary of State, who, after passing a preliminary competitive examination, were trained in the theoretical and practical knowledge of their profession—first at a University, and subsequently under one of the best practical telegraph engineers of the day. A final qualifying examination had then to be passed before the successful candidates, after signing a covenant with the Secretary of State, were sent out to take up their duties in India. This system prevailed till 1878, since which date all candidates for the Department have to pass through the course at the Royal College of Indian Engineering at Cooper's Hill.

After a short and unsuccessful trial of a tariff proposed by Colonel Douglas, based on a fixed fee, representing fixed expenses common to all messages, supplemented by a charge varying with the distance, on the 1st October, 1868, the universal charge of one rupee for 10 words (code messages double) was introduced. This tariff remained in force—with certain concessions made in 1870 in respect to the address, in which three words were counted as one word—until the adoption of the rate of one rupee for eight words, with free address; which rate, with a modification introduced in 1882 of "Urgent" and "Deferred" telegrams, permitting of the charge being doubled or halved at the option of the sender, is in use at the present day.

Early in 1866 Colonel Robinson went to England on sick certificate, and during his absence of two years, first Colonel Glover, R.E., and then Major Murray, acted as Director-General. When Colonel Robinson returned to India, in 1868, he was accompanied by the late lamented Louis Schwendler, in view to instruction of the signalling staff in electricity and magnetism, and the technical details of their profession generally. To this talented gentleman is due the placing of the working of the telegraph upon a thoroughly scientific basis, and the stimulation of the signalling staff to that interest in their work which has, in a most marked manner, led to the high state of efficiency of the present day. One of the first tasks undertaken by Mr.

Schwendler was the compilation of a series of "Testing Instructions," and the introduction of an elaborate system for testing insulators and localising faults on lines. The results which attended his efforts were remarkably successful. Speed and stability of long-distance telegraphy were perfected to an extent that enabled India to acquire and maintain her position as a link in the chain of international communication between Europe and the Far East. In the introduction of his system of scientific testing Mr. Schwendler was ably seconded by Mr. (now Professor) W. E. Ayrton, by whom an account of the methods adopted was communicated to the *Journal of the Society of Telegraph Engineers* for March, 1873. It may not be out of place here to note that to Mr. Schwendler, more than to anyone else, the residents of Calcutta are indebted for the magnificent Zoological Gardens which now form so attractive and instructive a feature in the City of Palaces. Appreciation of his services in this respect is marked by a granite obelisk, with a medallion portrait, which since his death has been erected in his honour in the Gardens.

In 1870 the experiment of establishing combined Post and Telegraph offices, with both duties in charge of the postmaster, was tried at two stations—Mussoorie and Naini Tal. The experiment failed signally, and Colonel Robinson said of it: "The fact is, the two duties are totally dissimilar, and to endeavour to combine them is as extravagant as to yoke together a race-horse and a cart-horse; . . . the good principle of division of labour is destroyed, and there is nothing as a set-off." This opinion is curiously illustrative of how new departures fail from being in advance of their time; for it is entirely owing to this system of combined Post and Telegraph offices that the extension of the benefits of a telegraph to small towns all over India has, in recent years, been made possible.

To Colonel Robinson is due the inauguration of a system for training British soldiers in electric telegraphy, and keeping them in training by giving them employment in telegraph offices. A start was made in this direction in 1868, and the great value of the military signaller was first proved in 1873-74, when there was a

famine in Tirhoot and the resources of the Telegraph Department were taxed to their utmost. That the Department did good work in connection with this famine is evidenced by the fact that 541 miles of temporary line were erected and 13 offices opened in 35 days. Had it not been for the military signallers, who were utilised to replace and set free civilians at various offices in different parts of the country, it would scarcely have been possible to meet the sudden demands made upon the signalling staff.

During the period 1871 to 1873 great improvements were made in the design and manufacture of the material used in the construction of the lines. It is to Colonel Mallock, who was employed at this time in England in supervising the supply of telegraph stores for India, that this improvement is due. To his talents and energy, combined with a thorough practical knowledge of Indian requirements, the Department is indebted for the high class of posts, brackets, wire, and insulators that are now generally in use.

The introduction of strong steel wire rendered possible the construction of long spans, by which means the trouble caused by the constant failure of river cables was in many instances avoided. Either by the use of tall masts, or by taking advantage of precipitous banks, some very long spans were put up over rivers. In 1873 a span was made across the Kistna River at Bezvāda, in the Madras Presidency, which measured 5,070 feet from post to post. This span is still in existence and is probably even now the longest in the world.

Through communication by telegraph between England and India was first established in January, 1865, by the Turkish route, with a tariff of £5 for 20 words. Delays and errors in messages were, however, the subject of universal complaint, with the result that in 1866 a Parliamentary Select Committee was appointed to take evidence on East Indian communications. This ventilation of the question resulted in the establishment of *two* additional routes: that known as the Indo-European—through Persia—was opened in January, 1870, and that *via* Suez and Aden to Bombay in March of the same year.

Although originally constructed by officers of the Indian Telegraph Department, the Ceylon lines were worked as an independent system under the Ceylon Government until 1869, when they were placed directly under the control of the Indian Government. This arrangement continued until 1880, when they were transferred back again, and since then the Ceylon telegraph has been quite separate and distinct from the telegraph of India.

Colonel Robinson's tenure of office was marked by a very large extension of the telegraph system, and by a remarkable improvement in the efficiency of the service. At the time of his death, in 1877, there were, roughly, in operation 18,000 miles of lines, carrying 40,000 miles of wire, with 234 offices—this in addition to 484 railway telegraph offices technically supervised by the Department. The revenue for 1877-78 is given as 3,322,252 rupees, and the expenditure 3,140,124 rupees. Like his predecessor, Colonel Robinson designed and introduced several new patterns of insulators, all of porcelain, protected by various descriptions of iron hoods. Under Mr. Schwendler's auspices duplex telegraphy was introduced, and the regular system of testing and localising faults before referred to put in force. The insulation of the lines was greatly improved, and the distances worked direct enormously increased. In fact, the Indian telegraph, which, though it made so promising a start, had fallen somewhat behind, was brought up abreast of the times.

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# ABSTRACTS.

## C. HEIM—THE EFFECT OF E.M.F. ON THE INSULATION RESISTANCE OF A CABLE.

(*La Lumière Electrique*, Vol. 38, p. 67, 1890.)

Three cables were experimented on—*a*, plain gutta-percha core, and *b* and *c*, lead-covered cables. The E.M.F. employed could be varied up to 470 volts. When measured with 52 volts, and then with 460 volts, the insulation was in every case lower with the higher E.M.F.—*a* 6.6 per cent. lower, *b* 2.9 per cent., and *c* 5.3 per cent. The following table indicates the general tendency of the experiments:—

Cable.	Volts.	1'.	5'.	10'.	15'.
<i>a</i>	53	7,500	9,530	10,100	10,540
"	213	7,200	9,370	9,950	10,490
"	470	7,050	9,015	9,570	10,000
<i>b</i>	21	2,290	4,495	6,480	8,215
"	213	2,185	4,280	6,290	7,955
"	470	2,180	4,190	6,090	7,670
<i>c</i>	53	14,750	29,800	40,500	47,700
"	213	13,500	29,200	38,100	43,950

The last four columns show the resistances in megohms after electrification for 1, 5, 10, and 15 minutes respectively.

Some tests were also taken on (*d*) a 19/18 electric light lead, 150 metres long, run on woodwork, in the usual way. It was insulated first with a covering of incombustible tape, then with tape impregnated with gutta-percha, and tarred cotton over all; also on (*e*) a No. 20 bell wire, insulated with paraffined cotton, and also run on woodwork.

Volts.	<i>d.</i>	<i>e.</i>
2	·60 Ω	·120 Ω
20	·58 „	·095 „
120	·50 „	·085 „

The field magnets of a Schuckert dynamo were also tested—first at a temperature of 17.2° C., and afterwards when the machine had been running for six

hours and the magnets were warm (temperature not stated); current in magnet-coils, 1,800 amperes per square inch.

	2 Volts.	20 Volts.	102 Volts.	400 Volts.
17·2°	17 $\Omega$	7	4	2·7
Hot	...	·75 to 1	0·6	0·5

### A. PALAZ—RECENT RESEARCHES IN ARC LAMP PHOTOMETRY.

(*La Lumière Electrique*, Vol. 37, p. 417, 1890.)

The author goes into the subject at considerable length, and reproduces several tables of measurements on numerous types of lamps, by MM. Wedding, Marks, Rousseau, and others. He considers that the mean spherical intensity may be expressed within about 10 per cent. by the formula,

$$S = \frac{H}{2} + \frac{M}{4},$$

where H is the horizontal and M the maximum intensity. The author gives the calculated and observed values of S for a considerable number of lamps.

For lamps running steadily at 40 to 50 volts, the maximum intensity may be found within 10 or 20 per cent. from the current by the formula,

$$M = 160 C + 3 \cdot 2 C^2 \text{ candles.}$$

*E.g.*, a 10-ampere lamp =  $1,600 + 320 = 1,920$  candle-power.

### POTIER and PELLAT—ELECTRO-CHEMICAL EQUIVALENT OF SILVER.

(*Journal de Physique*, Vol. 9, p. 381, 1890.)

This is a determination based on current measured by the absolute dynamometer devised by one of the authors. Its indications are believed to be correct to within ·05 per cent.

The anode was a thimble of silver about 1-10th inch diameter, immersed about  $1\frac{1}{2}$  inches; the cathode, a concentric silver cylinder, immersed about 2 inches. The electrolyte was a neutral 15 per cent. solution of silver nitrate. The electrodes, on removal from the bath, were washed in distilled water, and dried in a vacuum for 12 hours before weighing. The current was measured by inserting in the circuit a known resistance and a variable rheostat, the E.M.F. at the terminals of the former being kept constant, and exactly balancing a Clark cell of which the E.M.F. had been previously determined through current measurements with the dynamometer. The form used and recommended consisted of mercury covered with mercury sulphate, and above this a 15 per cent. solution of zinc sulphate, in which is the zinc plate. The advantages claimed are that it is much easier to make, that the polarisation is reduced to one-sixtieth, and the temperature coefficient to one-half, that of the paste form of cell.

The current was about .1 ampere, about .8 gram of silver being deposited in about two hours. The results of two determinations were 1.1188 and 1.1195 milligrammes per coulomb, giving a mean of 1.1192. Previous determinations are—

Kohlrausch...	...	...	1.1183
Rayleigh	...	...	1.118
Mascart	...	...	1.1156

# MERCADIER and CHAPERON—NEW RADIOPHONIC APPARATUS.

(*Journal de Physique*, Vol. 9, p. 336, 1890.)

The authors wished to find the best substitute for selenium, as the high resistance of a cell of that metal—some 300,000  $\omega$ —and its great variability with time, make it unsuitable for practical use. The principal substance experimented on was silver sulphide, prepared in three different ways. (a.) A sheet of silver was dusted over with flour of sulphur, and then heated; the excess of sulphur burnt off, and on cooling, a sheet of sulphide about 1-250th inch thick detached itself. This possessed radiophonic properties almost equal to sulphide prepared by method c. It was the only one that polarised; after having been traversed by a current it acted as an accumulator, giving out a current for some hours. It had a crystallised velvety surface. (b.) Sulphur and silver were fused together, poured out, and, while hot, rolled down to about the same thickness as a. Thus prepared, the sulphide appeared almost metallic. It was easily electrolysed, but did not polarise; it possessed but feeble radiophonic properties, and was of no practical use. (c.) A silver plate was placed in a solution of twice-crystallised sodium sulphide in distilled water, and sulphur electrolytically deposited on it. The film was then detached by warming the plate. Films were obtained 6 cm.  $\times$  3 cm. and 1/100 mm. (.0004 inch); they were chemically pure, and crystalline on the surface, and possessed radiophonic and thermo-electric properties to a much higher degree than either a or b. This form of sulphide does not polarise under the action of very feeble continuous currents, but increases in resistance like selenium, returning to its original value, however, in a few minutes after the current stops. It gives excellent results with alternating currents of about 100  $\sim$ .

Sulphide of tin, phosphide of zinc, and oxide of copper were also investigated, but are mechanically unsuitable.

The film when obtained was placed on a thick sheet of asbestos, and two helices of either iron, platinum, or silver were pressed firmly on to it. Fused contacts are most undesirable, as they only remain good for a day or two at the best. The above three metals are almost the only ones which were found to give a durable instrument. The resistance of this form of "cell" is much less than that of a selenium one, varying from 4,000 to 20,000 ohms. When continuous currents are used the E.M.F. at the terminals should not exceed 1.50th volt. It is sensitive to all radiation from the ultra-red to ultra-violet, and, owing to the extreme thinness of the film, it is instantaneous in its action. (A good radiophone should fall to about half-resistance when a paraffin lamp is brought within 8 inches

of it.) It is especially sensitive to ultra-red rays, the interposition of a small alum cell much reducing the effects. A sulphide of silver cell may also be used instead of an ordinary thermopile, and has the advantage of giving much sharper indications, the spot at once returning to zero when the cap is replaced. The resistance of the cell can be easily measured by the telephone and alternating currents, as the latter have no effect unless so strong as to spark across. Light has about the same effect whether alternating or direct currents are used. When the instrument is employed as a photometer or a thermopile, the E.M.F. at its terminals should not exceed 1-1,000th volt.

### **G. CHAPERON**—MEASUREMENTS BY ALTERNATING CURRENTS AND THE TELEPHONE.

(*Journal de Physique*, pp. 481, 484, 485, 1890.)

This refers to the method used to measure the resistances of the photophonic apparatus above mentioned. The ordinary form of metre bridge was used, but instead of the usual sized wire, one of G.S. .04 mm. (1.6 mils.) diameter, 100 to 150  $\omega$  per metre, was employed, or one of platinum iridium .02 mm. (.8 mil.), and 500 to 1,000  $\omega$  per metre. The great advantage of the metre bridge is that the capacity and inductance of the two arms are both practically zero. The resistances in the other two arms of the bridge,  $x$  and  $R$ , were about 100,000 to 200,000 ohms, that of the telephone receiver about 150 to 200  $\omega$ . To increase the sensitiveness, it was not used direct, but through a small transformer, the primary of which was about 5,000  $\omega$ , and the secondary the same resistance as the receiver itself. The resistance coils ( $R$ ) were not wound double in the ordinary way, but in layers, alternately right and left handed: this was found to reduce both inductance and capacity to a minimum. Double-wound coils were quite useless.

### **Professor J. A. EWING**—CONTRIBUTIONS TO THE MOLECULAR THEORY OF INDUCED MAGNETISM.

(*Phil. Mag.*, Sept., 1890, p. 205.)

The theory of magnetism as advanced by Weber is that the molecules of the magnetic metals, Fe, Ni, and Co, are always magnets, and that the process of magnetising consists in turning them from many directions to one direction. The author brings forward what may be called a molecular working model of a magnet on this theory, and shows that very many of the known facts of magnetism can be imitated with it. The model consists of a number of magnetised pieces of steel wire about one-tenth of an inch diameter and 2 inches long; a slight indentation is made in the middle of each with a centre-punch, and it is then balanced on the point of a sewing needle, thus forming a small compass needle. The magnet is slightly bent in the middle, so as to bring the centre of gravity below the point of support, and the lower end of the sewing needle is fixed in a small leaden block. A considerable number of such pivoted bars are then placed on a board on which lines are drawn to facilitate regularity in grouping. The model can, when desired, be magnetised by a coil surrounding it. The magnetic force applied is too weak to affect the magnetism of the bars individually. It alters their align-

ment only, just as magnetic force alters the alignment of Weber's molecular magnets. When grouped, either regularly or otherwise, and then disturbed and left to come to rest free from external magnetic force, they assume a form having no external magnetism; but they do not arrange themselves in closed chains.

If they are now subjected to a magnetising force,  $\mathfrak{H}$ , gradually increasing from zero, the first effect is to produce a *stable* deflection of all members, and to cause a small resultant moment in the group as a whole, increasing approximately as  $\mathfrak{H}$ . If  $\mathfrak{H}$  is now removed, the magnets, not having been deflected beyond their limit of stability, return to their original positions—there is no residual magnetism. If  $\mathfrak{H}$  is increased beyond a certain small limit, hysteresis comes into play, as group after group becomes unstable and breaks up, the members forming chains more or less in the direction of the lines of force; and a further small increase of  $\mathfrak{H}$  is sufficient to break up all, or nearly all, the groups. During this period the moment of the system as a whole increases very rapidly, and if  $\mathfrak{H}$  be now removed, a very large proportion of the moment which the group has acquired remains—residual magnetism. The chief facts of permeability, retentiveness, and hysteresis are therefore at once explicable by supposing that Weber's molecular magnets are constrained by no other forces than those due to their own mutual magnetic attractions and repulsions. Hysteresis, then, is not the result of any quasi-frictional resistance to molecular rotations; it occurs whenever a molecule turns from one stable position of rest to another through an unstable condition. Magnetic viscosity is well shown by grouping the magnets (preferably provided with air vanes to damp their vibrations) in a regular order, and applying a magnetising force. When this reaches a critical value one of the outside members swings slowly round, then (without increase of  $\mathfrak{H}$ ) its next neighbours follow suit, and the disturbance slowly spreads through the whole group. Several other effects of magnetism can also be imitated; for example, the supports of the magnets may all be placed on a thin sheet of rubber, which can be stretched longitudinally or transversely to obtain Villari and similar phenomena, several of which are mentioned.

# **Professor J. PERRY—APPROXIMATE DETERMINATION OF THE INDUCTANCE OF A COIL.**

(*Phil. Mag.*, Sept., 1890, p. 233.)

This is an approximate method of determining the coefficient of self-induction or inductance,  $\lambda$ , and time constant,  $\lambda/R$ , of a coil of wire from its linear dimensions. If

$a$  = mean radius of coil in centimetres,

$b$  = length (axially) in centimetres,

$c$  = depth of winding in centimetres,

$n$  = number of turns,

$\lambda$  = inductance,

$$\text{then} \quad \lambda = \frac{n^2 a^2 \times 10^{-7}}{1.84 a + 3.0 b + 3.1 c} \text{ secohms.}$$

This is very approximately true so long as  $\frac{c}{a}$  and  $\frac{b}{a}$  are less than  $\frac{1}{2}$ .

It is also shown that the time constant of a coil of given volume, wound with

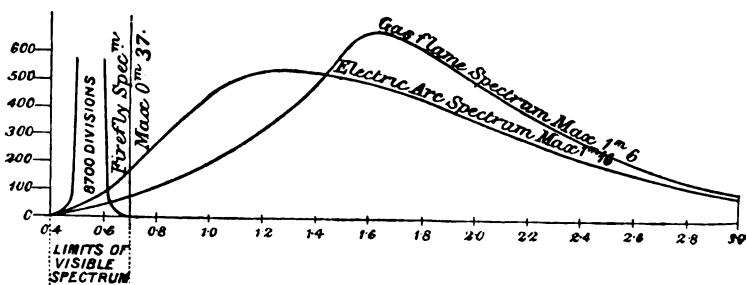
a given size of wire, is nearly proportional to  $\frac{a b c}{a + 2(h + c)}$ . To diminish the time constant it is therefore about twice as effective to increase either the length of the coil or depth of the winding as to increase the mean diameter.

### LANGLEY and VERY—THE CHEAPEST FORM OF LIGHT.

(*Phil. Mag.*, Sept., 1890, p. 260. Cf. Dr. Oliver Lodge, "*Modern Views of Electricity*," pp. 256 et seq.)

The object of this memoir is to show, by the study of the radiation of the firefly, that it is possible to produce light without heat other than that in the light itself; that it is actually effected now by nature's processes; and that these are "cheaper" than our industrial ones to a degree hitherto unrealised; "cheapness" being the ratio of light produced to the power expended to produce it. No sensible heat accompanies the firefly's light, any more than need accompany that of the Geissler tube. It is usually assumed that the light is produced without the invisible heat that accompanies our ordinary processes, and this view is greatly strengthened by the authors' experiments on fireflies and other noctilucous insects. Numerous experiments, both spectroscopic and thermometric, are described, which show conclusively that in the light emitted by the firefly and other insects the entire energy used is expended in producing vibrations whose wave-lengths lie wholly within the limits of the visible spectrum, instead of an enormous percentage of the power being lost in producing only waves of greater length which are useless. In the electric arc only about three to four per cent. of the energy expended in the arc really goes in the production of light, the remaining 96 per cent. giving merely valueless heat. In the Argand burner the loss is far greater. The calorimetric measurements were made with the well-known bolometer devised by the author. As an indication of the extreme delicacy of this instrument, it is pointed out that the quantity of heat actually measured when experimenting with a firefly was such that, had the bolometer been replaced by a mercury thermometer, the latter would only have been raised 1-400,000th degree centigrade. The electrical particulars of this instrument are not given.

Subjoined are curves showing one unit of heat displayed successively in spectrums of gas flame, electric arc, and firefly; the areas of the three curves being



equal, but that of the last being cut short. The useful work is the area from 0.4 to 0.7, all the rest being wasted in invisible vibrations (heat).

**F. UFFENBORN**—TEMPERATURE COEFFICIENT OF SWAN LAMPS.

(*Beiblätter*, Vol. 14, No. 7, p. 666.)

Two lamps were sunk in an oil bath, the temperature of which was varied from 20° to 300°, and their resistances measured at different temperatures; using only one Leclanché cell, with a large resistance in series with it, so as to avoid heating the lamp by the current. The two lamps gave a temperature coefficient between the above limits of  $-0.513$  per cent. and  $-1.056$  per cent. respectively.

**FR. VOGEL**—THE BACK E.M.F. OF THE ARC.

(*Beiblätter*, Vol. 14, No. 6, p. 539.)

The author refers to the dissociation of gases in a gaseous discharge, first pointed out by Schuster. In a steady arc work is done (a) in maintaining the carbon points and intervening air space at a constant temperature: this the author considers determines the true resistance of the arc; (b) in vaporising the carbons; and (c) in dissociating the hydrocarbons and other gases formed. That against which the last portion of work is done is, he considers, of the nature of a back E.M.F., and should be a valuable means for determining atomic heats of combination.

**LUMMER and BRODHUN**—COMPARISON OF THE HEFNER AMYL-ACETATE LAMP WITH THE STANDARD CANDLE.

(*Zeitschrift für Instrumentenkunde*, No. 10, p. 110, 1890; *Beiblätter*, Vol. 14, No. 8, p. 776.)

Two Hefner lamps were compared with the German standard candle, and gave the values  $.863$  C.P. and  $.841$  C.P. respectively. The latter was one made by Krüss. In the course of the determinations experiments were made as to the reliability of a small glow lamp ( $2.4$  C.P.), the current through which was kept perfectly constant, as a secondary standard. This was found to have altered only  $.4$  per cent. after 154 hours', and  $1.2$  per cent. after 211 hours' burning. For the short periods required such a lamp can therefore be considered as a perfectly constant sub-standard.

# LIST OF ARTICLES

## RELATING TO

# ELECTRICITY AND MAGNETISM

Appearing in some of the principal Technical Journals during the Months  
of NOVEMBER and DECEMBER, 1890, and JANUARY, 1891.

### BATTERIES.

- E. ANDRÉOLI—The History of Secondary Batteries.—*Lum. El.*, vol. 38, pp. 369, 423, 516, 558, vol. 39, p. 221.
- G. ROUX—Indicator for the Amount of Charge in an Accumulator.—*Bull. Soc. Int. El.*, vol. 7, p. 390.
- GRAWINKEL and STRECKER—The Use of Accumulators in the Berlin G.P.O.—*El. Zeit.*, vol. 11, p. 629.
- OBERBECK and EDLER—The E.M.F. of Cells.—*Ann.*, vol. 42, No. 2, p. 209.
- E. LÉVAT—Comparison of the Total Work done by a Cell with the Total Chemical Energy expended.—*Ann.*, vol. 42, No. 1, p. 103.

### LIGHT AND POWER.

- G. RICHARD—Descriptions, mostly illustrated, of (about) 140 different Dynamos.—*Lum. El.*, vols. 38, 39.
- W. MENG—Central Station at Verona.—*El. Zeit.*, vol. 12, p. 25.
- F. GÉRALDY—The Use of High-Tension Direct Currents.—*Lum. El.*, vol. 38, pp. 254, 553, 603.
- E. DIEUDONNÉ—Arc Lamps.—*Lum. El.*, vol. 29, p. 61.
- E. MEYLAN—Meylan-Rechniewski Meter.—*Bull. Soc. Int.*, vol. 7, p. 446.
- A. HESS—Resistances for Large Currents.—*Lum. El.*, vol. 38, p. 605.
- E. PIAZZOLI—Coupling Alternators in Parallel.—*Lum. El.*, vol. 38, p. 481.
- L. BAUMGARDT—The Treatment of Characteristic Curves.—*El. Zeit.*, vol. 11, p. 670.
- GRAWINKEL and STRECKER—Apparatus for Determining the Shape of the Current-Wave of a Machine.—*El. Zeit.*, vol. 12, p. 6.

### ELECTRO-METALLURGY AND CHEMISTRY.

- ANON.—Manufacture of Aluminium at Pittsburg.—*Lum. El.*, vol. 39, p. 29.
- G. RICHARD—Electro-Metallurgy of Aluminium.—*Lum. El.*, vol. 38, p. 201.
- ANON.—Electro-Bronzing of Iron and Steel.—*Lum. El.*, vol. 38, p. 331.
- A. SCHNELLER—Electrical Manufacture of Ozone.—*El. Zeit.*, vol. 11, p. 589.
- H. MOISSAN—Electrolytic Preparation of Fluorine.—*Lum. El.*, vol. 38, p. 401.
- Dr. G. GORE—The Greater Effect of First Quantities of Electrolytes on Volta-Electro-motive Force.—*Phil. Mag.*, vol. 30, p. 483.
- G. MENGARINI—Electrolysis by Alternating Currents.—*Lum. El.*, vol. 38, p. 541.
- F. PASCHEN—The Time required to set up an E.M.F. between Mercury and an Electrolyte.—*Ann.*, vol. 41, pp. 801, 899.

**TELEGRAPHY AND TELEPHONY.**

- E. ZETSCHE—Multiple Telegraphy for Private Lines.—*Lum. El.*, vol. 39, p. 17.
- ANON.—Disturbances of Telegraph Lines by Lightning Currents.—*Lum. El.*, vol. 39, p. 83.
- T. VALLANCE—The Baudot and Munier Automatic Systems.—*Jour. Tel.*, vol. 14, p. 289.
- C. HAUBTMANN—Cassagne's Steno-Telegraph.—*Lum. El.*, vol. 39, pp. 11, 72.
- E. MASSIN—Tests on Capacity and Self- and Mutual-Induction on Overhead Wires.—*Ann. Tel.*, vol. 17, p. 499.
- E. MERCADIER—Note on Loudness of the Telephone as affected by Diameter of Diaphragm and Strength and Form of Magnet and Coils.—*C. R.*, vol. 112, p. 97.
- S. KALISCHER—The Telephone. Residual Magnetism.—*Ann.*, vol. 41, p. 484.

**MAGNETISM.**

- A. MARIANINI—Magnetisation by Condenser Discharge.—*Lum. El.*, vol. 39, p. 41; *Nuovo Cimento*, vol. 27, p. 156.
- T. MOUREAUX—Values of Magnetic Elements on Jan. 1, 1891.—*C. R.*, vol. 112, p. 37.

**STATIC ELECTRICITY.**

- HILLAIRET—Effects of Lightning on a Transmission of Power Line.—*Ann. Tel.*, vol. 17, p. 558.
- A. PALAZ—Recent Lightning Protectors.—*Lum. El.*, vol. 38, p. 472.
- C. ZENGER—Electric Discharge in a Dusty Atmosphere.—*Lum. El.*, vol. 38, p. 251.
- E. BRANLY—Variation of Resistance of Metallic Powders under the Influence of Static Discharges.—*C. R.*, vol. 111, p. 785.

**INSTRUMENTS AND MEASUREMENTS.**

- Dr. M. EDELMANN—Small Wiedemann Galvanometer.—*El. Zeit.*, vol. 11, p. 669.
- BEETZ—Lecture Galvanometer.—*El. Zeit.*, vol. 12, p. 27.
- U. BARBIERI—Heating of Copper Wires by Current.—*El. Zeit.*, vol. 12, p. 30.
- P. CARDANI—The Temperature of Wires carrying Current.—*Lum. El.*, vol. 627; *Nuovo Cimento*, Aug., 1890.
- L. CHATELIER—Effects of Heating on the Resistance of Steel.—*C. R.*, vol. 112, p. 40; *Lum. El.*, vol. 39, p. 145.
- J. BERGMANN—The Use of the Induction Balance with a Galvanometer and Disjuncter.—*Ann.*, vol. 42, p. 90.
- E. ELSAS—Measurement of Resistance by the Differential Inductor (Transformer) and Telephone.—*Ann.*, vol. 42, No. 1, p. 165.
- G. H. ZAHN—Resistance of Bismuth as Measured with Direct and Alternating Currents.—*Ann.*, vol. 42, No. 2, p. 351.
- G. WIEDEMANN—The Determination of the Ohm (First Part).—*Ann.*, vol. 42, No. 2, p. 227.

## THEORY.

- Professor A. GRAY—The Magneto-optical Generation of Electricity.—*Phil. Mag.*, vol. 80, p. 494.
- S. SHELDON—Magneto-optical Generation of Electricity.—*Lum. El.*, vol. 38, p. 293; *American Journal of Science*.
- C. DECHARME—Analogy in the Propagation of Heat and of Magnetism.—*Lum. El.*, vol. 39, p. 51.
- LEPSIUS—Action of the Arc on Gases.—*Berichte. deuts. Chem. Gesells.*, vol. 23; *Lum. El.*, vol. 39, p. 129.
- J. TROWBRIDGE—The Motion of Atoms in Electric Discharge.—*Phil. Mag.*, vol. 80, p. 480.
- SHELFORD BIDWELL (Physical Society)—Selenium Cells.—*Nature*, vol. 43, p. 262.
- J. SWINBURNE (Physical Society)—Alternate-Current Condensers.—*Nature*, vol. 43, p. 263.
- T. H. BLAKESLEY—Determination of Work done on Iron Cores by Alternating Currents.—*Nature*, vol. 43, p. 116.
- S. ARRHENIUS—Conduction of Electricity by Hot Saline Vapours.—*Ann.*, vol. 42, p. 18.
- K. R. KOCH—The Increase of Friction at the Surface of a Polarised Electrode. The Occlusion of Gases.—*Ann.*, vol. 42, p. 77.
- D. BOS—Change of Volume of Dielectrics under Charge.—*Beibl.*, vol. 41, p. 1120.
- M. ASCOLI—Connection between Elasticity and Resistance in Metals.—*Beibl.*, vol. 41, p. 1124.
- ST. PAGLIANI—Seat of E.M.F. in a Cell.—*Atti. R.A.S., Torino*, p. 509, 1890; *Beibl.*, vol. 41, p. 1140.
- A. RIGHT—Photo-electric Convection.—*Beibl.*, vol. 41, p. 1167.
- H. HERTZ—The Fundamental Equations of Electro-Dynamics of Moving Bodies.—*Ann.*, vol. 41, p. 369.
- E. PFEIFFER—Variable Resistance of Distilled Water.—*Ann.*, vol. 41, p. 894.
- J. H. POYNTING—Discharge of an Imperfect Non-Conductor.—*Proc. Birm. Phil. Soc.*, p. 68, 1890; *Beibl.*, vol. 14, p. 1119.
- E. BLATTNER—Waste Heat in Glow Lamps.—*Beibl.*, vol. 41, p. 1172.
- L. ARONS—Experiments with an Electrically Polarised Platinum Mirror.—*Ann.*, vol. 41, p. 473.
- J. STEPHAN—Electrical Oscillations in Straight Conductors.—*Ann.*, vol. 41, p. 400.
- K. WAITZ—Wave-Lengths of Electrical Oscillations.—*Ann.*, vol. 41, p. 435.
- E. ELSAS—Electrical Oscillations in Unclosed Circuits.—*Ann.*, vol. 41, p. 833.
- E. LECHER—Researches on Resonance Phenomena.—*Ann.*, vol. 41, p. 850; *Lum. El.*, vol. 39, p. 89.
- SARASIN and DE LA RIVE—Hertz Oscillations in Air.—*Beibl.*, vol. 14, p. 1200.
- J. MIESLER—Quantitative Photographic Determination of Period of Oscillation.—*Beibl.*, vol. 41, p. 1162.
- E. LECHER—Determinations of S.I.C.'s by Hertz Vibrations.—*Ann.*, vol. 42, p. 142.

**SUNDRIES.**

- A. BIGAUT—Platinum in Electrical Industry.—*Lum. El.*, vol. 38, p. 295.  
—SERULLAS—Gutta-Percha.—*Lum. El.*, pp. 351, 406, 462, 524, 570, 612.  
G. SCIAMA—Report on the New Customs Tariff for Electrical Goods.—*Lum. El.*, vol. 38, p. 433.  
CHASSAGNY and ABRAHAM—Thermo-Electricity.—*C. R.*, vol. 111, p. 732.  
A. WALTENHOFEN—The Action of the Thermopile as an Accumulator.—*Lum. El.*, vol. 38, p. 275; *El. Zeit.*, Oct. 17, 1890.  
P. MARCILLAC—The Orecchioni Electric Torpedo.—*Lum. El.*, vol. 39, p. 205.  
H. SUTTON—The Problem of Seeing at a Distance.—*Lum. El.*, vol. 38, p. 538.  
G. DUMONT—Electricity for Safe Railway Working.—*Bull. Soc. Int. El.*, vol. 7, p. 398.
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# JOURNAL

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The Two Hundred and Sixteenth Ordinary General Meeting of the Institution was held at the Institution of Civil Engineers, 25, Great George Street, Westminster, on Thursday evening, February 12th, 1891—Mr. WILLIAM CROOKES, F.R.S., President, in the Chair.

The minutes of the Ordinary General Meeting held on January 22nd, 1891, were read and approved.

The names of new candidates for election into the Institution were announced and ordered to be suspended.

The following transfers were announced as having been approved by the Council:—

From the class of Associates to the class of Members—  
S. W. Baynes.

From the class of Students to the class of Associates—  
Charles Priest.

Donations to the Library were announced as having been received since the last meeting from the Director-General of Telegraphs in India; Messrs. Whitaker & Co.; Mr. C. H. W. Biggs, Member; and Mr. Latimer Clark, Past-President; to whom the thanks of the meeting were duly accorded.

The PRESIDENT: We will now discuss the paper by General Webber read at the last meeting, on “Electric Lighting from “Central Stations, with special reference to the Chelsea System.”

Professor  
Thompson.

Professor SILVANUS P. THOMPSON [*communicated*]: One of the most interesting points in the working of the Chelsea station which Major-General Webber has brought under our notice is the use of continuous-current transformers, or motor-dynamos, as an adjunct to the battery supplies, to be used whenever the demand on any sub-station exceeds the safe output of the batteries. I have often urged the importance of this particular mode of transformation, and have a great belief in its importance in the future. About three years ago I presented to the Physical Society the results of an investigation into the possible losses and reactions in such a transformer. In that investigation I found that the effect of the transformer was to convert the volts at the terminals of the primary and secondary circuits in a ratio depending only on the respective number of windings on those two circuits of the armature, and independent both of the speed of the machine and of the magnetism of the field magnet; and at the same time the effect of the resistance of the primary (or fine-wire winding) was to virtually add to the resistance of the secondary (or coarse-wire winding) a resistance equal to that of the primary winding reduced in proportion to the square of the relative number of windings in the two circuits. If the weights of copper used in the two windings are equal, then (if it be assumed that insulation in each case occupies the same relative thickness, or the same total volume in each case) the effect of the presence of the resistance of the primary winding is simply to double the resistance of the secondary winding. Hence a system of such continuous-current transformers may be used to supply lamps at a constant voltage by simply slightly over-compounding the generating dynamo.

I also pointed out that the mutual induction between the two windings aided greatly in reducing the self-induction of the several sections, so that the sparking at the brushes would be far less at both the primary and secondary commutators than would occur in the case of ordinary dynamos or motors of equal output. Further, that as the mechanical driving of the revolving part took place in a symmetrical manner, the coils which drive being embedded between the coils which are driven, no trouble was likely to arise from the lubrication of bearings.

The experience of the Chelsea station is most valuable in relation to these various points, and shows that the continuous-current transformer is a most reliable agent for the distribution of electric power and light. It ought not to be forgotten in this connection that the earliest suggestion for distributing power by such a means came from Mr. Lane-Fox.

I should be glad if General Webber could state whether my theoretical views on one point are borne out in the running of these machines at Chelsea. According to my calculations the transformation of electric pressure ought to be independent of the speed, the latter being in turn dependent upon the magnetism of the field magnets alone. Does the speed of these machines, as a matter of fact, become any greater or less when they are transforming a heavy current? Do the reactions of this compound armature weaken the field magnet or strengthen it? or are they so nearly balanced that the field and speed remain constant independently of the currents going through the machine?

Professor FORBES: There seems to be somebody required to fill the gap, and I will make a few remarks. I am sure we all wish very much that the different people connected with supply stations would give us as full accounts of their experiences as General Webber has done here. It would add very much to our information, and I am sure we are all very grateful even for this instalment from one company—that with which General Webber is connected. I will begin by making a few remarks about the machinery and apparatus employed, and I would say that, on first reading this paper and looking at the diagrams which have been appended to it, to some persons it would look as if the whole system were so complicated by ingenious automatic devices as to be practically unworkable, and I should not be surprised if that were a very general impression amongst those who have heard and read this paper. My own impression was such when I first heard of this system; but I may say that when I saw the thing in action, and saw how much simpler the actual machinery is than the diagrams, that impression faded away entirely. Undoubtedly there is always some superintendence given to these sub-stations, but the amount of superintendence required with

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the use of these automatic appliances is enormously less than if we were simply dependent upon human intelligence to superintend them. I think Mr. King deserves the greatest thanks for the way in which he has worked out a system so complete in every detail, as everyone must acknowledge it to be, even if they do think it is too complicated. It is very interesting to hear some account of actual experience with these continuous-current transformers, and the remarks of Professor Thompson are very interesting on this subject. The language in this paper is a little indefinite as to the types of apparatus that are here used. So far as I can make out, there are two distinct types—one, which was the original one, which has been apparently most worked and another, apparently under trial, which has been made at Wolverhampton. With regard to the second one, General Webber has given us an indication of the efficiency of the Parker apparatus at different loads, and it comes out that at 40,000 watts it attains an efficiency of 92 per cent. I should like to ask exactly what is meant by the efficiency which comes out so high as 92 per cent. Does it mean that 92 per cent. of the electrical energy supplied to the motor terminals of the dynamo is transformed into electrical efficiency given out by the dynamo terminals of the machine? Of course, if that is the case, that is a very high efficiency indeed; and although I know that is from the tests of the manufacturers, still it would be desirable to have it precisely stated what the efficiency is that is being measured. Professor Thompson mentioned in his few remarks that the ratio of the electro-motive force of the secondary to that of the primary ought to be in proportion to the ratio of the number of turns, and independent of the field-magnetism and the speed of revolution. That occurs to one as naturally the case, and yet I fail to see that it is carried out in this particular machine. General Webber says, in this machine there are 200 turns on the primary and 80 on the secondary, which is a ratio of  $2\frac{1}{2}$  to 1. But the winding of the primary is for 540 volts, and of the secondary for 108 volts, which is 5 to 1. There seems to be a little discrepancy here. It is possibly from a misunderstanding, on my part, of the description

given of the machine, but I am sure it will be interesting to others as well as to myself if General Webber will show where the trouble lies. The paper teems with facts and practical experience, which I am sure will be most welcome to all electrical engineers. And, amongst other things, I am very much struck with the data he gives us as to the waste of peroxide plates, on page 74, where he gives us some idea of the probable duration of these peroxide plates. Experience is so different on that point that these facts, I am sure, will be hailed by all of us with great satisfaction. Major-General Webber has done me the honour of referring to the work which I did many years past in preparing the way for electrical distribution, when we had no electrical distribution, and when we felt some difficulty as to how we were to proceed. He has said that certain of my conclusions were certainly to be accepted, whilst two of them might not necessarily be adhered to—namely, that the distributing boxes should be nearer to the central station than any lamp fed from it; and that small groups of lamps should be fed from separate wires. The qualification as to the second I admit. Actual practical experience shows that it is not a necessity, although it seemed at that time it might eventually be so. The other one—that the distributing box should be nearer to the central station than any lamp fed from it—is undoubtedly a principle of economy, where economy of metal is the one thing that we are dealing with. At the time my Cantor Lectures were delivered, we had no satisfactory high-pressure system at all, and we were dealing only with low-pressure systems, involving a large quantity of copper, and economy of copper was the one thing we had to aim at, and undoubtedly it was very necessary that that principle should be adopted. It is still necessary that it should be adopted in cases that admit of it. But I agree that many cases occur in actual practice, where districts are not perfectly regular, where the streets are not arranged in rectangular blocks, like the model city the diagram of which he has put on the board, and there are many cases where it is desirable to come back towards your central station; but as a general rule the principle is right—that you ought to have

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your distributing box closer to the station than any lamp fed from it.

There are one or two little points that one differs from considerably, and one I happened to mark is this: General Webber says, "I am no advocate for the other and equally unreasoning extreme of procedure by which light is wasted foolishly in frosting bulbs . . ." I think that is rather a matter for the artistic taste than for the electrical engineer to discuss, but my own opinion is most strongly opposed to General Webber's. In private houses, where artistic display and absence of strain on the eyes are requirements, frosted bulbs are most necessary. My own experience of the naked filament is that it is painful to the eyes where you are continually meeting it; and in a house which has to be artistically lighted I would not allow any naked filaments to be seen. Moreover, there is another scientific reason why it is not desirable, and that is, very often a room lighted by frosted globes is better lighted than a room lighted with clear globes. With frosted globes, if there be no dirt upon them, the loss of light would be next to nothing. Frosting the globe simply causes the light to suffer a series of refractions which do not cause any absorption at all. The only loss of light in a clean frosted globe is due to the insignificant absorption of the light in passing through a short path in glass. The only real loss in a dirty frosted globe is due to repeated absorptions at dirty surfaces, which absorb a certain amount of light. But we judge the general effect, in a lighted room, by contrast, and we contrast the brightness in the lighted parts of the room with the brightness of the lamp itself; and if we have the naked filament, the intense brilliancy of that filament makes the rest of the room appear very dark. There is another little point in General Webber's paper with which also I disagree entirely—that is, the advocating of 8-candle-power lamps instead of 16-candle-power lamps, and the astounding statement that a room lighted with 8-candle lamps is equally well-lighted as one illuminated by 16-candle lamps. His comparison of Brooks's Club with the Devonshire Club is a misleading one. One has much smaller rooms, and the other consists chiefly of very large rooms indeed. Very large rooms lighted with 8-candle

lamps, instead of the same number of 16-candle lamps, would, I am sure, impress General Webber as being far less efficiently lighted. The lighting of a room must not be judged by the general impression as you come into it. No person could go out of the Devonshire, after seeing the light there, and examine Brooks's, and say which is the better lighted by simply looking at it. The simplest way is by examining small print or something of that sort, and seeing the facility with which you can decipher "Bradshaw," or any other small-type book. Another point I would also differ from General Webber in is—it is a matter of no very great importance, but he seems to lay some stress upon it—as to the regulation admitted by the Board of Trade, that the voltage of different houses lighted from a central station may vary. Major-General Webber seems to think this was owing to the fatherly action of the Board of Trade, and that they thought they would assist supply companies to get over some of their difficulties. As a matter of fact, I think I am right in stating that this permission was granted in consequence of an application of the St. James's and Pall Mall Electric Light Company. I was acting as their consulting engineer, and agreed thoroughly to the plan of admitting different voltages to different parts of the district. General Webber says it is quite possible that, in this plan, when you add one or two new houses, the house which required 98 volts yesterday may require 100 volts to-day. That is perfectly true; but in a district like the St. James's and Pall Mall Company's district, which is a very compact district, in which the routes of the mains are perfectly clearly determined beforehand, you know perfectly well that if you are going to adopt a certain density of current—say 500 amperes to the square inch—you will lose 5 volts in every 200 yards, and you design your station to have that density of current as a maximum, and if you determine the voltage of houses simply by their distance from the central station, and allow loss at the rate of 5 volts for every 200 yards, you will be perfectly secure. If the density is less than that, the houses will be more regularly supplied, will vary less, but the variation will never be greater, and the voltage required by any house never greater than what is dominated by

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that means of working. At the same time, I know numbers of cases in America where that has been tried, and where it has absolutely failed, simply owing to their carrying their mains in an irregular fashion over the whole district, and not always taking the shortest route to the central station, and becoming inextricably confused after a short time by branching off lines from the feeders, by tapping the feeders, or by diverting the mains back towards the central station.

There is a great deal of information in this paper in connection with the laying of underground mains, and a great deal of practical experience of great value to us all. Different systems have been compared. I am not going to criticise any one of these systems which have been spoken of here. I think our feeling at the present time is that we should encourage the full development and trial of every system of laying underground mains we can, to wish them the heartiest success, and reap the results of experience when we have had experience of their working. I agree with General Webber that, especially in the initial stages of lighting a district, it is most desirable that we should have a system of laying mains by which we can draw in and out cables—that is to say, we can begin by putting in small cables when the supply is small, but that we should be perfectly prepared to extend the lighting by putting in larger cables as soon as the supply demands it. It is very undesirable that we should have to take up the streets when more houses are taking light. Among other systems mentioned is that adopted considerably in America—described by General Webber as layers and faggots of wrought-iron tubes buried in concrete. These have been largely laid down in New York. They are very efficient, I know, but the chief objection to them is their very great expense. There is one place where General Webber compares an insulation, such as that of the Edison mains, with one of bituminous compound, which I presume means the Callender-Webber system. I object entirely to the bitumen compound being spoken of as an insulating material, and I am sure General Webber will not say it is looked upon as such. It must be looked at simply as a means for drawing in the wire. It is not

waterproof; water leaks through it extremely easily. If you weigh a lump of it, and put it in water for a considerable time, and then weigh it again, you will find the weight considerably increased. Another defect of that material has always seemed to me to be its liability to fracture. It is desirable that whatever the insulated cables are covered with should act as a protection from picks and steam rollers and other nuisances, and it seems to me that a friable material like this is not the best thing we should wish for. In fact, in America they have gone to extreme pains to make their protection absolutely secure from such injuries. However, I believe General Webber in conversation pointed out to me many reasons why he thought bitumen concrete was quite strong enough, and I am sure we shall all be interested to hear what he has to say upon the subject. General Webber has spoken about the Callender solid bitumen insulation, which is being used pretty considerably, which I have seen laid down in several places, notably in Liverpool and in the place he has mentioned—Bath—where I was last week inspecting the mains. This consists of iron troughs in which the insulated cables are laid on bridges of wood, and the troughs then filled up with pitch or bitumen. With regard to this General Webber says: "The line, once down and well laid, need, so far as one can see, never be disturbed, except by accident or injury, and a sufficiency of capacity for probable future demands is secured without heavy cost." Now I think it must have gone against the grain with General Webber to so write that—to say that a cable can be thoroughly embedded permanently and not be capable of being examined for faults afterwards. I am sure his telegraphic experience would have gone against the desirability of such a system, and I can only give evidence contradictory of what he has given here as to the facts of the case. Last week, in Bath, I found the insulation resistance of one of their circuits 44 ohms, and that of another 58 ohms. Of course these were serious faults, and would undoubtedly be corrected immediately; but that shows that this system of insulation is not infallible, and that you must expect faults in it, and leads one more strongly to the opinion that a

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system must be one easily got at to remedy faults.\* I would like just to mention an extremely interesting means of detecting faults I saw in use in Bath. I daresay it is familiar to many of you here, and I believe it belongs to the Callender system. It consists of a number of iron plates arranged on a trolley, which runs over the mains on the road. The plates are at right angles to the line of cable, and round these plates is a coil of wire connected with a telephone. The trolley is moved over the line; an intermittent current is passed through the cable, from cable to earth—the Thomson-Houston arc light machine being found amply discontinuous enough for the purpose—and the trolley is laid over the mains, and the person testing listens with the telephone as he passes along the line. The change from loudness to silence is extremely marked when you pass the fault. If a Mordey alternator is put on in place of the Thomson-Houston machine, the effect is equally good, only in that case it is a more musical hum, instead of a jar; and when the two are worked along the mains at the same time, it is perfectly easy to distinguish the Thomson-Houston arc from the Mordey alternator, and when you come to the fault you can tell whether the cable is connected with the Thomson-Houston or with the Mordey machine. It is extremely interesting. I will not make any remarks upon the other systems of laying mains, simply because I have a sort of feeling that I would be beginning to find fault somewhere, and I do not want to. But I may say I am very pleased to find General Webber in one place endorsing the opinion which I expressed somewhat strongly a few years ago, and which I have never found reason to throw aside, of the enormous care required with lead-covered cables, to secure their perfect water-tight character. He mentions a case in which great trouble has been caused in Paris in this way, and that is thoroughly in support of the opinion which I formed from careful investigation, and which I still hold.

Mr Crompton, I have no doubt, will give us some information about the London Chamber of Commerce circular, which, as

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\* I find that I was mistaken in believing that the faults were in the mains. I am now assured that they were found to be in the lamp-posts.

General Webber stated, has rather startled us lately. There are many points in it of interest; but I daresay this is hardly the place to enlarge upon the full reasons of the various details in that circular, but it is one which I have no doubt will receive great attention in the future.

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In conclusion, I will only say I am sorry to find—undoubtedly it was inevitably the case—that the chief thing we want to know about the Chelsea installation is not given in this paper. We have all of us admired the ingenuity of it, the facilities it gives for a steady light, and all the rest of it, and we are all sure that if we were lighting our houses we would like to have storage batteries in thoroughly good working order, always delivering current at a perfectly constant potential. But electrical engineers wish to know what is the cost of the whole thing. I have no doubt it is difficult at the present time to deal with that; but if we could have had such information with regard to the Chelsea system as we undoubtedly shall in the course of a year or two, it would have added enormously to the value of the paper.

Mr. CROMPTON: On account of receiving the corrected proof of General Webber's paper only a few hours ago, I have not been able to thoroughly examine it, and to check the figures that have been altered. General Webber has brought before us a subject which ought to be of extreme interest, and I am somewhat surprised that it has not provoked a more lengthy discussion. He describes the details and partial results of the working of a system which is unique, as it is the only central station supplying electric energy on a considerable scale, passing the whole of its supply through storage plant, and using the storage at distant points as a means of transforming the electric energy at high pressure into the lower pressure required for the lamps in the houses. Three years ago I read a paper here on central station lighting,\* and compared the merits of the alternating transformer system with the battery transformer system. I then pointed out that accumulators were a most desirable, valuable,

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\* Journal of the Institution, vol. xvii., p. 349.

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and trustworthy adjunct to any system of distribution, and that the chief defect of the alternating transformer system is that accumulator storage cannot be used. I have just read over the discussion which followed my paper, and I find that several of the speakers, including Mr. Kapp, stated that I was then the only friend that accumulators had. He was surprised at my courage in recommending their use. At that time the only friends that accumulators had besides myself were the manufacturers of the accumulators themselves. At that time I advocated their use in a modified way; that is to say, I used a certain proportion of storage plant sufficient to give about one-third of the supply at times of maximum demand for the purpose of increasing the output of the moving machinery at that time of maximum demand, and again for supplying the consumers during the long hours of small demand, when it is most desirable that the moving machinery should be stopped. I think that I had considerable foresight when I decided on that method, for further experience and careful examination of the various systems at work has shown that it is the right method. Most of the specifications that have been issued for the supply of generating plant for large towns on the Continent of Europe have followed more or less on the lines I have laid down. The experience that I have had since the time of writing that paper has only confirmed my views. I have figures in my possession which show clearly that there is no advantage in providing accumulator plant of a greater capacity than is sufficient to supply one-fifth of the total output of the 24 hours, if we take the heaviest 24 hours' demand in the middle of winter. This proportion of accumulator plant is of course only about one-tenth of that advocated in the paper before us. Returning again to the discussion on my own paper, Mr. Parker asked me why I had not referred to the Colchester installation, and I then said that the greatly increased first cost, and hence cost of upkeep, of the accumulator plant which the Colchester system entailed, put it quite out of comparison with the two systems I was then discussing. The answer that I then gave to Mr. Parker applies equally to the more highly developed Chelsea system which we

are now considering. I repeat that the present system cannot be brought into comparison, as regards either first cost or cost of upkeep, with the other systems of electrical distribution with which it is in competition; that all the advantages that are given by the use of storage plant are given by the modified use that I have always advocated—viz., that the storage plant should not be greater than sufficient to supply 20 per cent., or at the outside 30 per cent., of the total output required during the longest winter day. Mr.  
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I next call General Webber's attention to the most important defect in his paper. The time for description of new systems, without any accompanying data which will satisfy an educated audience like ourselves as to their commercial merits, has gone by. Considering the time that the Chelsea system has been at work, General Webber, if he considers the present time a fitting one for describing the system and comparing its merits and demerits with those of other systems, ought to have furnished us with working accounts to a sufficient extent to enable us to compare the original cost of the plant, the cost of its upkeep, and what I may call the engineering cost of working it. I say that he gives us no figures at all that are of any use to us, to really enable us to compare his system with other systems. I admit that there are figures giving the results of isolated tests, or of running a portion of the plant; but long experience has shown me that such figures are of no value, as in most cases the costs so obtained would require to be doubled in order to agree with the figures obtained from the monthly or quarterly accounts. There could be no possible objection to his publishing such portions of the accounts as would interest us. Such figures have already been published by the St. James's Company, and by the Kensington Company; and if General Webber had furnished us with figures, we should have been able to institute comparisons which would have been of real value in determining the merits of the system under consideration. Such portions of the accounts which are common to all systems, and which relate to the cost of management, rents, rates, and the like, are not interesting to this audience, and it is this portion which companies do not care to

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publish. The author states that only 28 per cent. of the energy is lost in his mains and in the conversion in the accumulators. I cannot understand how he arrives at this figure.

Even if his accumulators were in extremely perfect order, and there was little or no waste of current, it would be extremely difficult to obtain such a result, even if we supposed the loss in distribution by the network is reduced to the lowest possible limit. The figures that would have been of real use to us in this would have been the actual number of units generated in the station in the given period, and the corresponding number of units actually paid for by the consumers.

On the cost of underground mains. It is a marvel to me how General Webber has arrived at the cost of my own system of underground culverts. If he had referred to my 1888 paper, he would have found figures differing widely from those he has given. In order that a correct comparison of costs may be made, I have prepared the accompanying table, which, in the first two

	EXTRACT FROM TABLES I. AND II., MR. CROMPTON'S PAPER OF APRIL, 1888.								WEBBER'S FIGURES, 1891, CALLENDER'S CASING.		CROMPTON, 1891, W.I. PIPES.		CROMPTON, 1891, CULVERTS.	
	I.—Callender.				II.—Culvert.									
	1/2"	1/4"	1/2"	1/4"	1/2"	1/4"	1/2"	1/4"	1/2"	1/4"	1/2"	1/4"	1/2"	1/4"
Callender casing, W.I. pipes or culverts ... ..	£ s. d. 0 11 4	£ s. d. 0 11 4	£ s. d. 0 11 4	£ s. d. 0 11 4	£ s. d. 0 11 4	£ s. d. 0 11 4	£ s. d. 0 11 4	£ s. d. 0 11 4	£ s. d. 0 11 4	£ s. d. 0 11 4	£ s. d. 0 11 4	£ s. d. 0 11 4	£ s. d. 0 11 4	£ s. d. 0 11 4
Surfaces boxes, 12 yards apart ... ..	0 2 3	0 2 3	0 2 3	0 2 3	0 2 3	0 2 3	0 2 3	0 2 3	0 1 11	0 1 11	0 1 11	0 1 11	0 1 11	0 1 11
Insulators & straining gear ... ..	...	...	...	...	...	...	...	...	...	...	...	...	0 0 7	0 0 7
Cables ... ..	1 11 7	0 15 9	...	...	1 11 0	0 18 0	0 1 11 0	0 18 0	...	...	...	...	...	...
Culvert ... ..	...	...	0 12 2	0 6 3	...	...	...	...	...	...	...	...	0 12 2	0 6 3
Extra if high-class I.R. insulation on cables... ..	2 5 21	9 41 6	10 1 0	11 1 19	9 1 6	9 1 19	5 1 6	5 1 6	1 6 5	1 6 5	1 6 5	1 6 5	1 6 5	1 0 7
	...	...	...	...	...	...	...	...	0 10 4	0 2 8	...	...	...	...
	...	...	...	...	...	...	...	...	2 9 9	1 9 1	...	...	...	...

columns, gives the comparative cost of Callender cable drawn into bitumen casings with a culvert system, in both cases employing the same weight of copper in the conductors. The only

alteration that has been made is that both have been corrected <sup>Mr. Crompton.</sup> for the three-wire system, so as to compare them with the figures now given by General Webber in the third column, and by myself in the fourth and fifth columns. I can guarantee the absolute accuracy of the figures I now give, as they are taken from the costs of considerable lengths of mains that have been recently carried out for the Notting Hill Company. The figures, therefore, represent the actual sums that we as contractors have received for the work. The table shows that when each of the two outside wires of the three-wire system have a cross section of  $\frac{1}{4}$  in., and the middle wire of  $\frac{1}{4}$  in.—i.e., a total of  $1\frac{1}{4}$  in. of copper—the Crompton culvert system costs £1 6s. 6d. per yard, against General Webber's £1 9s. 9d.; and even at half this section—viz., when all three wires have the same cross section of  $\frac{1}{4}$  in.—the culvert system costs £1 0s. 7d. per yard, against General Webber's £1 6s. 9d.; and the difference becomes still more striking if high-class india-rubber insulation is used on the cables.

I take this opportunity of saying that I consider Mr. Callender's bitumen casing to be a most excellent invention, which has been of great service in underground distribution; but, as the table shows, General Webber is wrong in supposing that there is any great difference in cost between laying it and wrought-iron pipes. A matter of fact, the cost of providing separate cable-ways of a definite size is about equal in both cases. I have myself used considerable quantities of Callender's casing, and intend to continue to use it. I think that the bitumen itself is an exceedingly durable material, and may compare favourably in this respect with the wrought-iron pipes. I have taken special steps to protect the wrought-iron pipes from corrosion; but it will be many years before we actually know what the real life is of wrought-iron pipes so protected. The French gas companies have used wrought-iron pipes served with yarn steeped in bitumen, and I am informed that their life is as long as that of cast-iron pipes. It must be recollected that we meet with many cases where, on account of the large number of gas, water, and other pipes irregularly disposed under our main thoroughfares, we cannot carry our mains across the road at all in

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bitumen or anything in the form of a large trough. We are obliged to use separate wrought-iron pipes threading in among the other pipes.

Referring to General Webber's criticisms on the circular issued by the London Chamber of Commerce, and signed by me, this circular was issued with the intention of obtaining criticisms and remarks, so that the best form of circular on which large electric central station statistics could be filled in might be decided on. I, however, am perfectly prepared to substantiate the proposal that in calculating the cost of upkeep and renewal an annual sum amounting to 1 per cent. on the first cost of such imperishable materials as concrete, copper, and glass is quite enough to set aside for repairs and renewals, and that with covered cables the extra risk of upkeep is fairly represented by increasing this sum to 5 per cent. Far more competent authorities than I am have considered that  $\frac{1}{2}$  per cent. on the cost of the concrete, culverts, copper, and insulators would be sufficient to keep them in thorough repair; and, on the other hand, there are many who consider that 5 per cent. is not a sufficient sum to cover the repairs of continuously insulated cables, which have to be occasionally withdrawn from bitumen casing or from iron pipes; so that, on the whole, I believe that in giving these figures I rather favour the continuously insulated system.

Mr. King.

Mr. FRANK KING : I should have been pleased to have heard a few more comments, adverse or otherwise, on the system of electrical distribution which General Webber has brought before the notice of the Society. I think there are only one or two points of interest with which I have to deal. First, the life, or the probable life, of the secondary battery. From the details contained in the paper, it appears that 1,870 cells have been in use, some of them for a year and nine months. The whole battery may be taken to have been in use for an average period of 18 months. These 1,870 cells each contain 31 ordinary L type E.P.S. plates, which were made at Millwall Works. At the present moment, out of the whole, the renewals that have actually been supplied are 30 *single* plates, or *two* peroxide sec-

tions, or, say, 1-19th of 1 per cent. in 18 months. Even in these Mr. King. two cases where renewal has become necessary, the maintenance which has been required has been practically the result of accident. The amount of attention given to the batteries is not very carefully arranged. The method is somewhat as follows:—Each battery is tested two or three times in each night to see that it gives equal discharge with its fellows. Should it happen that any battery does not take its proper proportion of the load, it is marked, and examined next day, and the faulty cell is found, taken out, and repaired and replaced. The result, if the fault should happen to be ignored, or left for a few days owing to pressure of work, is that the plates in the damaged cell will very quickly be sulphated beyond effective repair, and they will have to be renewed. The question is, What is the ultimate necessary provision for depreciation which the cells are likely to need when worked in the manner shown in the system as designed by me?

In 1888 I published a pamphlet in which I made the statement that the Electrical Power Storage Company would, at that time, and in similar circumstances, guarantee to maintain these accumulators in working order for  $12\frac{1}{2}$  per cent. of the first cost. That was agreed to by the Electrical Power Storage Company upon my advice, and I am quite sure the results obtained from the experience at Chelsea will show that I looked forward to a very fair margin of profit to the Electrical Power Storage Company on such a contract. General Webber has stated that the staff of the Chelsea Electricity Supply Company have estimated that the positive plates will last at least three years. Should that be the case, the maintenance of the positive or peroxide plates represents 7·3 *per cent. per annum* of the first cost, at ordinary market prices. Should the negative plates last six years (and probably they will last a considerably greater period), the maintenance of the negative, or spongy lead, plates represents 3·9 *per cent. per annum*. Altogether, 11·2 per cent. *per annum* is an ample allowance where E.P.S. batteries are charged by a normal constant current. I believe the positive plates will last a great deal longer than three years, and

Mr. King. I hope that when all that are at present working have been renewed the Society will have full particulars, and whether my predictions are right or wrong will then be shown. I am sorry to have heard no remarks about the regulation of pressure on the mains of the Chelsea system. I claim that, extraordinary fogs excepted, the system of regulation of pressure, or E.M.F., on that circuit is one of the best, if not the most perfect, in existence. As a matter of fact, I believe, recording voltmeters are kept in every sub-station, and you have in the illustrations to the paper one or two curves of pressure taken from the records of the various stations. You can see there quite clearly the effect of the working of the automatic apparatus in taking out, or putting into, the circuit of discharge the counter E.M.F. cells. And in this connection—namely, that of maintenance of pressure—I should like to speak of the continuous-current transformer. I believe that these transformers can be applied to low-tension distribution circuits so as to act *as additional sub-stations*, as well as additional aids in supplying maximum or occasional abnormal demand. If the secondary, or generator, armature of the transformer is connected to a feeder or set of feeders which are distinct and separate from all the other feeders at the station, the pressure delivered by the transformer *at the point of junction with the distributing mains* will be not more than one-half or three-quarters of a volt in excess of the normal pressure required on those mains, provided always that the automatic apparatus at the station governs the E.M.F. from the discharge from the accumulators. I believe, in some of the more difficult problems which have occurred at Chelsea, where there has been a long line of distributor with a fairly large demand, the curve or fall of E.M.F. along that distributor has been greater than was desirable; and I believe that has been one cause of the complaints of which the public have heard. So soon as that condition of affairs was brought to my knowledge, I suggested that one of the feeders should be detached from the general system at one of the stations during period of maximum demand, and the feeder then applied to the transformer. Immediately that was done, complimentary

letters were received by the Chelsea Company, and the pressure is now somewhat over the 100 volts. That has continued, although in one place there was an objection by the company that pressure was a volt or so too high. I said, "Attach another feeder in common with the first, and let the second feed in the direction of another part of your circuit where the consumers complain that they are only getting 97 or 98 volts." Since that has been done, I believe the pressure has been practically constant—that is to say, varying only 1 or  $1\frac{1}{2}$  volts either way.

There was just one point in Professor Forbes's remarks about the transformer manufactured at Wolverhampton. I think it will be quite clear, although I am not responsible for the statement, that the figure 200 is a misprint for 400. I do not think, with the exception of calling particular attention to the cost of renewal of plates, and the maintenance of pressure by the automatic regulator, I need say anything about the remainder of the paper. I would like to impress the importance of the latter upon electric lighting engineers generally, and for this reason: it is very well to say you can have manual labour, and regulate pressure at any point just as you please; but if you are obliged to limit the conductors to commercial dimensions, it will be found that regulation by hand is a matter of extreme difficulty, and requires constant watchfulness on the part of the engineer in charge, whereas with the use of the automatic regulator and the continuous-current transformer, as worked at Chelsea, the engineer neither has to pay wages for regulation, nor does he have any anxiety on the subject. Under these circumstances, then, I believe I may fairly claim that the automatic regulation of pressure is absolutely perfect in its working and utility.

Mr. W. HOWARD TASKER: Before the discussion closes, I should like to say a few words upon one or two points. First, I would like to draw attention to a remark which fell from Professor Forbes. He stated, with regard to the Callender cables, and the method of using them for the mains at Bath by embedding them in bitumen, that he found the insulation resistance extremely low on two of the mains he had recently tested, namely, 30 and 40 ohms respectively. In Chelsea, I may

**Mr Tasker.** say that our experience with these cables is very different; the whole of the mains are laid down in network, and our tests include the feeders, the distributors, the service lines, the consumers' wires, and all their lamps and fittings. In a recent test upon a section of the mains, with 3,000 lights on the circuit, we obtained 20,000 ohms at a testing pressure of 100 volts. I think that is an extremely good result. We have had no occasion to withdraw a single cable of Callender's since the mains were laid down, except for the purpose of enlarging them, the insulation being very perfect. I think this is information that may be welcome to you all. Mr. King has referred to the recording voltmeters in use at Chelsea. We have at all the storage stations recording voltmeters. Throughout the 24 hours of each day the work done and the pressure are recorded. As regards meters, I might say one or two words. The meters we have employed from the commencement are the Aron meters. We find the working most satisfactory, and we had at the end of December 250 meters in position, and very few of them have given us any trouble. In fact, the maintenance of meters, including men's time, winding twice a month, and reading the meters, and the material used in the upkeep of them—an item which is very small—is covered by less than 4 per cent. I should very much like to draw attention to the diagram on page 70 (Fig. 10): it is very instructive. You see that in 1889, in the first three months shown—viz., October, November, and December—the line of increase in load is very marked. It was rising rapidly month after month until the end of December; then it gradually fell again. The identical thing, only in a more marked manner, occurs in the same months of the next year; showing that the greatest load increase goes on more rapidly at that time of the year than at any other in a residential district, so far as our experience in Chelsea and Kensington is concerned. The load remained for some time in the first quarter of the year somewhat constant, and in the summer time, of course, was very light.

**Mr. Mordey.** **Mr. MORDEY:** Professor Forbes has mentioned with approval an ingenious device used at Bath for finding faults in the underground mains. I think it is only fair that I should mention that

my friend Mr. Henry Sayers—who was at one time with the Brush Mr. Mordey. Company, and who has for three years been in charge of a large low-tension station at Oporto—wrote me two years ago that he used an arrangement on the same principle. Some time afterwards, Mr. Spagnoletti mentioned to me that he proposed to use a similar device at Paddington; and quite recently Mr. Heaviside showed me a very useful apparatus, on the same lines, that he had developed. I have reason for knowing that Mr. Heaviside applied the principle several years ago to an allied purpose. I am very much interested in what General Webber and Mr. King have said on the subject of dynamotors. I think we should use the word “dynamotors” for these direct-current transformers; it is a word of Mr. Swinburne’s, and is very expressive. It seems probable that where the distance to be covered is only a few hundred yards, it would be simpler, and perhaps not more costly, to work direct from the station, and to use mains with a loss not greater than is incurred in the present leads and dynamotors, with the advantage that no running machinery is required outside the station. I believe that, although such machines may be used very much in the future, it might be often almost as useful to work direct from the station in comparatively short distances, such as in Chelsea. I am glad to see that General Webber and the Chelsea Company have been converted to the transformer system. I knew it was sure to come sooner or later. It is coming, I suppose, in the form of direct-current converters. In a few years the direct-current converters will increase, and the reserve of batteries will diminish, and then the company will be consumed by unavailing regret that it did not put stationary converters in in the first place. One other point—as to the life of accumulators. It is always very interesting to have any information on the probable life of accumulators. But it would really be more important if we could get, in a practical form, a guarantee on the figure that Mr. King mentions. Maintenance at  $12\frac{1}{2}$  per cent. per annum is a very nice figure—many times greater, however, than is necessary with any other transformer; but we will let that pass. Can we get this practical thing—a guarantee from the makers of accumulators

**Mr. Morley.** that the cells shall be maintained at such a figure? No. If pressed for a guarantee, they will say that if the cells are worked under proper conditions, that figure is an ample one. But, unless they work the cells themselves, I think there is a disinclination on their part—or was some time ago—to give any guarantee at all, to take any responsibility; the reason being that as the accumulators are difficult to keep in order, except by those who understand them, they require that they shall be looked after by skilful people. Now the people who are skilful enough are not to be found outside the works of the makers of the accumulators, and not always inside, so that the figure is a little fallacious. I should rejoice to hear that accumulators are now sufficiently advanced to enable the makers to give a practical guarantee when the things are sold in the ordinary way of business, perhaps to be sent into the country or abroad, to be looked after by the engineers, and workmen who can be trusted to take charge of other electrical apparatus. Until this can be done, any mere statement as to what the depreciation ought to be is of very little value indeed to the industry.

**Major-Gen.  
Webber.**

**Major-General WEBBER:** At this late hour of the evening I cannot allude to all the points raised by those gentlemen who have discussed my paper. As regards Professor Thompson's question, I can only say that as we have always run the transformers at their fullest capacity, and the E.M.F. of the output has followed the varying potential at the terminals of the feeders, no test which would give a clear answer to his question has been made. But I may remark that what he said on the subject of the perfect balance of the armature when running is a very important feature in connection with the machines. In reply to Professor Forbes's remarks on the efficiency that was given for the two constant-current transformers—particularly the one marked the "Wolverhampton," by Elwell-Parker—I would like to state that the curve of that efficiency is one which, though not diagrammatically given, is described in figures sent me by the makers; but I cannot say that the working result has been equal to what was given by them. I have little doubt that that is what they obtained in the tests made at their works, but at the time this

paper was written and printed I had not the means of giving any confirmation, or otherwise, of them. The efficiency is certainly that which you would take in the ordinary way in proportion to the current which is received at the terminals of the primary, and that which is given out at the terminals of the secondary.

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As regards the question of frosting bulbs, I think Professor Forbes slightly misunderstood me. I cannot agree that there is no loss of light whatever by absorption in its passage through frosted glass. Nor can I admit that the only loss is due to the dirty surfaces caused by the accumulation of dust on the bulb itself.

In my paper I have assessed at many hundreds of pounds per annum, the loss by interference of obscure media between the eye and the object required to be illuminated. I have received from the Edison Company bulbs frosted on various portions of their surfaces, destined for particular positions, so that wherever the filament is exposed to the eye, it is intercepted by the frosted glass, but otherwise the glass is clear; and the use of such glow lamps is, I believe, an excellent demonstration of my view of the case. As regards my point in connection with the Board of Trade rule allowing companies to declare a varying pressure, of course I can perfectly understand and agree with Professor Forbes that where you can at the outset, when you lay down your system of distribution, provide for a current-density equal to what the maximum possible load will be, then my remark does not hold good; but it was intended to point out that in cases such as that I described, where you lay down a main of estimated capacity, or estimated for a consumption which may be very wide of what actually turns out to be required, you may find yourself under these conditions, having first declared a varying pressure along that main, in the awkward position of having to re-declare at other pressures, and having to obtain the permission of the local bodies to do so. I think Professor Forbes did not quite realise the condition I described, under which that facility is really of advantage in those parts of the areas near the barriers by which London is arbitrarily divided.

He speaks of bitumen as being not an insulator. [Professor

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Webber.

FORBES: Bitumen casing.] I thought he meant that bitumen was not an insulator. Bitumen casing, which is made of a concrete mass, is not an insulator, nor was it ever supposed that it would be so; but, as it happens, a result has been proved from its use that although you could not depend upon it for one moment as a structure into which you would draw bare copper, still there is an assistance to the insulation derived from it under certain circumstances. The embedded cable, which Professor Forbes remarked I probably praised against my grain, is spoken of by him chiefly in reference to its use with high-tension systems; and there I wish to say that under those conditions—that is to say, when the size of the conductor is not likely to be altered for some time, and there is no great difficulty in making ample provision for the future—then I would be the last in trying to deter those who wish to use that system from adopting such an economical means of laying down their conductors. In the interests of the industry it is very desirable that economy can be proved. In a large number of country towns in which it is desired that electric light should be economically established, I do not think that for some time to come any widespread means of distribution by primary conductors is likely to be adopted.

I come then to the remarks of Mr. Crompton, and I feel extremely obliged to him for the kind way he has spoken of the subject-matter of the paper, because in many ways I am indebted to him for varied information which I have derived from communications made by him from time to time in this room. It may be my fault not having recalled the paper he read three years ago, and to which he alluded, and my not having used the figures which he then gave. I should like, of course, to substantiate my figures, and Mr. Crompton says I cannot. Well, I have got out figures. I may be very wrong in my conclusions, but my 24s. which I show to be the cost of the subway which is used in many parts most successfully, as I have said, and constructed in a very admirable manner—my cost of 24s. per yard—is got out from building surveyors' figures, which I think are very fair.

				Per 100 Yds.			Major-Gen. Webber.
				£	s.	d.	
Trench, complete—sides, bottom, and top—exclusive of the stones of the footway, at 15s. a yard				...	75	0	0
Boxes, straining and intermediate	...	...	...	20	0	0	
Balks, insulators, and fixing	...	...	...	8	11	0	
Vestry charges, making good	...	...	...	9	6	0	
Removal of earth, &c.	...	...	...	10	0	0	
				£122	17	0	
Add 10 per cent.				...	12	5	9
				£135	2	9	

The above is subject, of course, to Mr. Crompton's correction; but if it is anything like correct, I do not think my figure, 24s. per yard, is too high. Then to that I add certain items I believe inherent to the system. That is to say, once in 100 feet the direction has to be changed, necessitating additional strainers, including the royalties of the inventor, involving a cost of, say, £10; and, lastly, on an average, once in 100 yards the necessity to resort to the use of 10 yards of tubes and covered cable, costing, say, £5 more.

In his comparison I do not think Mr. Crompton, if he will allow me to say so, has been any less unfair than he thinks I have been to him. He compares the cost of a wrought-iron pipe of  $1\frac{1}{4}$  in. or  $1\frac{1}{2}$  in. with a casing of 2 in. or  $2\frac{3}{8}$  in., and so on. Well, I am perfectly willing to admit, when you come to compare the cost of iron as against a  $1\frac{1}{4}$ -in. way in a case of bitumen concrete, the iron is as cheap; but when you come to  $1\frac{1}{2}$  in. or 2 in., and more, you find the difference is enormously the other way.

I have left out all the extra cost—which I believe is not small—of the work in straining the copper strip into position; so that I do not think, in comparing the two examples—namely, of a section of copper of  $1\frac{1}{4}$  in. in each case—that one costs very much more, or less, than the other.

Mr. Crompton deplored my reticence on certain points, and

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that I had not divulged certain figures, and he thought anyone who does not like to divulge their figures must necessarily be the advocate or father of an unsatisfactory system. I do not quite go all that way with him. Many of us have skeletons in our electrical cupboards which we do not care to divulge, and Mr. Crompton will acknowledge he has had experience of a good many of them. They start up in most extraordinary ways. I think I was fair to my audience. I enumerated a certain number of abnormal adverse conditions which may occur in any system for the supply of a commodity in daily life, almost inherent in the starting of any new industry. I did not for one moment attempt to conceal that these had occurred and existed up to the present time. I think anyone who has been through the experience of starting an electric light station, a system of distribution over an area in London, under the rules, regulations, penalties, &c., of the Board of Trade, and under all the difficulties which we encounter from local authorities, owners, occupiers, &c. (Mr. Crompton himself has been all through them), will see that in the first 18 months—the childhood of any system—there will be shortcomings and conditions which affect the figures—that is, the gross results—which it would be perfectly unfair to any system to go and tell, except with an explanation, which would be very long, and in the end liable to misinterpretation.

But I do give figures; and I gave what I thought was a very fair figure. I took the boiler efficiency: I gave you that, not under experimental conditions, as Mr. Crompton suggested, but under favourable daily conditions. I took the running of one of our plants under the most favourable daily conditions, over a considerable period of time. I then gave the loss by conversion, giving the meeting to understand that that loss was divided between what was due to the charging mains, and to the conversion, taking the average results with the two means. I also gave the loss up to the customers' terminals, and the cost of the coal per unit sold—1.1 of a penny. That is a fairly practical figure, and one which I think may be accepted and adopted as the truth, subject to the losses arising out of those disabilities to which I referred, and one we may all expect to arrive at in time; indeed, it is

a rather high figure, and I hope we shall arrive at something better. Anyone who has gone through the process of testing an electric light station will realise at once that the figure you are looking for, is, how much coal you are putting into your plant, and how much electricity you are selling, and how much you get for it? You do not take the isolated figures arrived at from the efficiency tests of the various parts as the means of compiling the results under the varying conditions of load during the hours of the day and during the seasons of the year.

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Now, as regards the point mentioned by Mr. Mordey. He and I were long ago associated together in the early days when transformers with alternating currents were brought to this country, and we have had many conversations over them as to their efficiency and so forth. But I do not agree with his remark that the system of conversion at Chelsea is on the same lines as that which we know him to be intimately connected with, and which he has most successfully put to work in various places. A question common to the two is no doubt the economical proximity of the "sources of supply," subject to the density of the consumption. That is the thing I had in my mind when I began to write this paper, the foundation of which was one I read before the British Association at Newcastle, in September, 1889. The question is, can we arrive at any rule on which you will establish how far apart these converting stations are to be, whether you use accumulators and their auxiliaries, continuous-current transformers, or ordinary alternating transformers? How far are they to be apart? and how far will that distance govern the size of the conductors you are going to lay underground in the first instance, and the size of the conduits in the second, to meet future demands? I have not arrived at any absolute conclusion myself on the subject, and it is one I should have liked to have heard discussed to-night, and one possibly we shall gain more experience of as time goes on, and particularly when any engineer has to consider what system of distribution he is going to adopt, according to the conditions of demand in various towns of the United Kingdom, according to the spending power of the population, and various other things which will govern his design. If

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the outcome of the consideration of this subject enables us to decide, under certain conditions of demand, within a certain superficial area, the distance apart for a given consumption we may place our converting stations, or "sources of supply," we shall have obtained information which, I believe, will not only regulate future procedure as regards many areas in which electric distribution is now contemplated, but will also settle the system of conversion which we shall be inclined to adopt.

A ballot for new members took place, at which the following candidates were elected :—

*Foreign Members :*

Robert McAllister Lloyd.		Henrik Munck.
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*Associates :*

William Edward Cross.		Herbert Charles Moberly.
George Heys Jones.		Alpheus H. Hardy Trott.

*Students :*

J. A. Ashby.		Graham T. W. Olver.
M. Beales.		J. E. Pierce.
R. J. Edwards.		A. B. Rigby.
Archibald H. Finlay.		T. E. Slaughter.
Robert Leigh Griffin.		C. W. J. Tennant.
Henry Leslie Harris.		W. G. Wallace.
F. O. Harrison.		S. E. Watson.
John Henderson.		Alan Williams.
H. A. Ling.		Henry Gurney Wood.
Thomas Herbert Minshall.		John Venner.

The meeting then adjourned.

The Two Hundred and Seventeenth Ordinary General Meeting of the Institution was held at the Institution of Civil Engineers, on Thursday evening, February 19th, 1891—Mr. W. CROOKES, F.R.S., President, in the Chair.

The minutes of the Ordinary General Meeting held on February 12th were read and approved.

The names of new candidates for election into the Institution were announced and ordered to be suspended.

The following transfer was announced as having been approved by the Council:—

From the class of Students to the class of Associates—  
Ernest Gordon Okell.

The following paper was then read:—

## TRANSFORMER DISTRIBUTION.

By J. SWINBURNE, Member.

The distribution of electrical power by means of alternating-current transformers can hardly be considered to have reached its final state, and though it may be somewhat difficult to say what system will be in general use ten years hence, there is little doubt that present practice will be largely superseded.

The method now in vogue is simple: it consists of running several independent dynamos, each with its own engine; each dynamo supplies a few high-pressure mains, which branch off, and supply transformers placed in the houses, or wherever light is required. This system, admirable for country towns or straggling villages, is hardly likely to be permanent anywhere else. It has many disadvantages. Except that all the dynamos and engines are in one building, it is nothing more than a collection of little independent installations. Each engine and dynamo supplies its own circuits, which are independent of all the others. In cases of a

breakdown of an engine or dynamo, and in many cases of the leads, there is a stoppage till new arrangements can be made at the station. In addition to this, great lengths of high-pressure conductor are required, and this leaves openings for breaking down of insulation. The introduction of high pressures in private houses is dangerous if not well managed, and is in any case expensive, demanding compliance with all sorts of regulations, rendered necessary for the security of the public. The greatest drawback to the present system, however, lies in the waste of power in the transformers themselves. For a long time transformers were supposed to have an efficiency of about 98 per cent. More will be said on this subject presently.

It must be borne in mind that in spite of the drawbacks of the present system of putting transformers in houses, it may be, and probably has been, advisable for electric light companies to begin on that system, even in such places as London; but I do not think such an arrangement will pay in the long run, and a time will arrive when the mains will have to be changed and a low-pressure supply system substituted. When an electric light company begins, it has to take on houses here and there, where it can get them; but if its district is populous and wealthy, it should soon be enabled to work on some low-tension or sub-station system; and then the question is whether it is not better to lay the low-pressure mains at first, instead of beginning on one system and changing to another. Probably none of the existing companies mean to change from one system to another. Their engineers have no doubt gone into the merits of the particular cases, and have come to their conclusions with great deliberation, and I do not think it well to offer any criticisms lightly.

#### PARALLEL RUNNING ACCORDING TO ORTHODOX HYPOTHESIS.

To begin at the beginning of my somewhat extensive subject, the first question is that of running machines in parallel. There are few matters of greater importance to engineers, and few on which opinions differ so widely. One says that machines will not run in parallel unless they have large self-induction; another that it is necessary to have iron in the armatures; another that

they will only run together if they have no self-induction, or as little as possible. Others say that there is no difficulty in running them in parallel. No engineer who employs direct currents denies the advantages of running machines in parallel; if independent running is advisable in alternate systems, it is equally advantageous in the case of direct currents, and direct-current stations should have separate circuits for each little district, each district being run by one dynamo. I do not think, therefore, independent running is advisable.

In 1884 Dr. Hopkinson showed that alternate-current dynamos could be run in parallel, or could be run as dynamo and motor, and even that a machine giving a high pressure could be run as a motor off a circuit of lower pressure. In his paper he assumes that machines have "self-induction." He points out that the self-induction varies, but that it would be impossible to work out the calculations if the "self-induction" were taken as variable. For some reason or other, Dr. Hopkinson's calculations have received little application. That is to say, engineers have not taken actual cases and worked them out. Calculations have been made in which the various quantities have appeared in the form of letters, and diagrams have been drawn in which they are represented by lines whose lengths have been chosen at random. The geometrical treatment of many problems of this kind shows most of the actions very clearly to students, but is not always of value in practical work, as in most cases some lines are so long and others so short that it would be difficult to avoid large errors. The result of this is that few of us, if any, have had a definite idea of how much "self-induction" a machine should have to make it work well in parallel with others. The general impression is that it is somewhat difficult to design a machine whose "self-induction" is large enough, and that most failures to run in parallel are due to deficiencies in this respect. I am using the term "self-induction" under protest, as I prefer to look at the question from another point of view. But whatever goes on in a dynamo, if the no load electro-motive force is treated as constant, a value for a constant coefficient of self-induction can be assumed which would produce results somewhat

similar to those found in practice. To overcome the difficulties arising from deficient self-induction in the armature circuit, Dr. Hopkinson has proposed to insert it artificially, in the form of choking coils, or to use induction couplers to compel synchronism. Without knowing that it had been done before, I described a similar arrangement to the Institution a few years ago.

In the spring of 1890 Mr. Mordey read a most important paper, in which he described some experiments on alternate machines. He argued from them that self-induction was deleterious; and that machines would run together best if they had none. In the discussion, Dr. Hopkinson pointed out that the experiments were quite in accordance with his theory, and that there was nothing that could not be foretold by it.

To begin with, it is necessary to get a clear idea of what is required for parallel running. It is not exactly the same as the case of alternating motors, neither is it the same as two machines of equal pressure coupled in parallel by themselves. If all the machines are coupled to two main conductors, we may consider them one at a time, and take each dynamo as coupled to a system whose pressure and variations of pressure are independent of it. Suppose the effective pressure to be 2,000 volts, for example: we have to consider a pair of mains with 2,000 volts, and we have to couple on a dynamo to add so many amperes to it. Such a machine must not be excited to give 2,000 volts on no load, being then thrown into circuit with full steam turned on immediately afterwards. It should be excited to give somewhere about half load with 2,000 volts at the terminals, and should then be thrown in. Steam can then be turned on, and the excitation raised to its normal value. The machine should be so designed that any excessive power that the engine may give shall not only not throw the machine out of step, but shall not increase the armature current to such an extent as to damage the insulation by excessive heating.

The power absorbed by a dynamo working on a parallel system is

$$P = \frac{B^2 r}{2 i^2} - \frac{A B r}{2 i^2} \cos 2 \pi n \tau + \frac{A B L}{2 i^2} \sin 2 \pi n \tau,$$

where  $A$  is the maximum station electro-motive force—that is to say, the station pressure is  $A \sin 2 \pi n t$ — $B$  is the maximum “impressed” electro-motive force in the armature,  $r$  is the armature resistance,  $i$  the impedance of the machine, and  $\tau$  the angle of lead. This equation is taken from Dr. Hopkinson’s paper. When the dynamo is doing its proper share—that is to say, under normal conditions—

$$B = I c,$$

where  $I$  is the impedance of the whole circuit when the machine is isolated, and working at full load on an external resistance, and  $c$  is the maximum value of the current. To find how much the driving power may increase for a small increase of current, we may differentiate with respect to  $c$ , and then find the value of  $L$  which makes the result a maximum.

$$c = \sqrt{\frac{A^2 + B^2 - 2 A B \cos 2 \pi n \tau}{i^2}}.$$

$$\cos 2 \pi n \tau = \frac{A^2 + B^2 - i^2 c^2}{2 A B}.$$

So,

$$\frac{dP}{dc} = \frac{A B r i^2 c}{2 i^2 \cdot A B} + \frac{A B 2 \pi n L}{2 i^2} \cdot \frac{d \sin 2 \pi n \tau}{d 2 \pi n \tau} \cdot \frac{d 2 \pi n \tau}{d \cos 2 \pi n \tau} \cdot \frac{d \cos 2 \pi n \tau}{d c};$$

and in the case of a dynamo working on resistance, or on a station load when supplied with just its right share of power,

$$\cos 2 \pi n \tau = \frac{A + r c}{B},$$

$$\text{and } \operatorname{cosec} 2 \pi n \tau = \frac{B}{L c}.$$

So,

$$\frac{dP}{dc} = \frac{1}{2} A + c r,$$

$c$  being the maximum, not the effective current. It will be observed that  $L$  has disappeared altogether.

This shows that for a very small increase of power the increase of current will be the same, whatever the self-induction of the machine may be. It would not matter if the machine had none. A machine with no self-induction would (Mr. Mordey notwithstanding) short-circuit the mains if the engine supplied too little power; in fact, at full load it would be giving its minimum

possible output, and any attempt to diminish this would be disastrous.

Discussing the question of a very small increase is not exhaustive; an engine may supply a large proportion more than its share of power to one machine. For instance, suppose it supplies twice the normal power. Many engines could not give such an excessive power, but in others which have expansion gear controlled by a speed-governor, it is easy to get double power if the station is running at a slightly slower speed than that for which the governor is set.

These matters are always made much clearer by taking practical examples and working them out. The great difficulty is to obtain accurate data. I should prefer to discuss such machines as those described in Mr. Mordey's paper read here a short time ago. Without wishing to say anything in disparagement of his excellent paper, I must point out that an account of experiments should contain full particulars of the machines on which the experiments were made. Such omissions are exceedingly common in electrical papers. Mr. Mordey has not given us the details of the field magnets, air space, armature coils, or exciting coils; he has not even given us the currents in the different experiments. I have found similar omissions whenever I have tried to gain information from any of the numerous experiments which have been made on alternate machines in America. Mr. Mordey does, however, say that about half the drop in his machines is due to resistance, and half to self-induction. The machines give an output of 2,000 volts and 20 amperes, and when fully excited give 2,200 volts at no load. This would correspond to a self-induction of 0.05 henry.

Suppose, first, that the self-induction were such that  $2 \pi n L = r$ : on increasing the power supply, the effective current increases evenly, and when the power supplied to the machine is doubled, the effective current is only about 44 amperes. It is necessary, however, that the machine should stand decrease of load, as well as increase, without injury. Keeping the same self-induction, let us decrease the load to zero with the full excitation on. The current gradually diminishes to just below 15 amperes, and then increases to just over 20 again.

These figures are extremely troublesome to work out from Dr. Hopkinson's equations, as the angle is so small that an error in about the sixth place of decimals will vitiate the results.

Many people prefer diagrams such as those used in mathematical books when dealing with harmonic variations. Mr. Blakesley has applied these diagrams to the discussion of alternate dynamos and motors, and Messrs. Kapp, Fleming, and others have applied them too. The chief drawback is that it is not always easy to draw such figures to scale. They are generally drawn with the values taken at random, to give ideas of the principles involved. So far as I know, no scale diagram of this sort has ever been published. In the case of the 40,000-watt machine such diagrams can be applied; but with 100,000 machines, which will probably be the standard size, the diagrams are almost illegible.

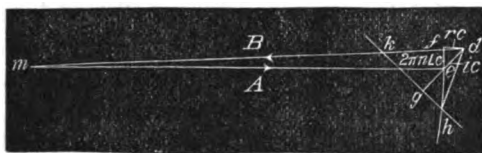


FIG. 1.

Fig. 1 is a diagram to scale. The lines are all rotating positively. The diagram is made as follows:—The pressure in the mains, A, is drawn first. Instead of the maximum, it is simpler to draw the effective pressure—in this case 2,000 volts. From the end  $e$ ,  $2\pi n L c$  is drawn at right angles to it to  $f$ ; and then  $r c$  from  $f$ , parallel to A; B is drawn to join the ends of A and  $r c$ , as shown. The lines are all pressures; but the instantaneous current is in step with the pressure  $r c$  and with the pressure A, so that the power is proportional to  $B r c \cos f d m$ . I use the expression “in step” to mean co-phasally synchronised. Thus two machines may be running at the same speed without being in step; just as soldiers may take the same number of paces per minute without being in step.

To find the current at double load, produce  $i c$  to  $g$ , so that  $d g = 2 i c$ . Through  $g$  draw a straight line cutting B at  $k$ , at



the frequency 80. This case cannot be shown clearly in a diagram, as  $r c$  is very small in comparison with  $2 \pi n L c$  or  $B$ . The result is, however, that the machine cannot possibly take double power from the engine; it falls out of step first. Now 60,000 watts is not a large output for central stations, and it is probable that 100,000 will be about the size adopted. The "time constant" of such a machine is considerably greater. We thus find that according to the self-induction hypothesis the difficulty does not lie, as commonly supposed, in getting enough self-induction in the armature; the trouble is that there is generally too much. But in machines from 25,000 to 50,000 watts it is exceedingly difficult to make dynamos that will not run perfectly in parallel, so great is the permissible margin of self-induction. It must be carefully borne in mind that so far the armature has been supposed to have a constant coefficient of self-induction, and the pressures of current are assumed to vary harmonically.

The orthodox hypothesis thus tells us that with ordinary sizes of machines it is exceedingly difficult to design them so that they will not run in parallel. Either, therefore, all ordinary machines run perfectly in parallel, or the assumption that alternating currents follow sine laws, that machines have coefficients of self-induction constant throughout each period, or that they have coefficients of self-induction constant for different armature currents, is too far from the truth to be useful; or, in legal language, one or some or any or all of them must be wrong.

#### PARALLEL RUNNING ACCORDING TO ARMATURE REACTION THEORY.

Last year I had the honour of reading a paper before this Institution on armature reactions in direct-current machines. In it I showed how the performance of large machines could be foretold; and the whole question of the effect of the armature current on the field magnets was discussed with some minuteness. I cannot pretend to treat alternate-current machines with the same exactness, but still a great deal is to be derived from the study.

It has sometimes been assumed that a machine has self-induction in the armature, and also that there are armature

reactions to be considered, and the two actions are treated as if their joint effects had to be taken into account. This view is, I submit, erroneous. Imagine a battery with its cells connected to a number of contact pieces, and imagine the middle cell connected to a terminal, to represent one terminal of the machine, and the other terminal represented by a rubbing contact which is moved backwards and forwards over the cell contacts by a crank and connecting-rod. This gives the analogue of a hypothetical machine with no self-induction. If a choking coil is put in circuit, the hypothesist's alternating machine would be produced. To get a constant coefficient of self-induction in a real machine would be almost impossible. If a coil passes between two cheeks of laminated iron, its coefficient of self-induction is constant. In a dynamo, however, half the iron is cut away, the remainder forming the pole-pieces, and this remainder is not laminated. Even if a dynamo gave a curve of sines at no load, therefore, the reasoning already given would not be accurate, as  $L$  would vary considerably from one part of the period to another.

The term "self-induction" is also used somewhat loosely. Engineers do not generally think of electrical inertia now. By a self-induction  $l$  in an electric circuit, is meant that if there is a current of  $1$  in it, it is interlinked with one line of induction. The idea of self-induction is naturally inseparable from the notion of magnetic induction being interlinked with the circuit. Another way of looking at it is to say that if the current increases there is a back electro-motive force proportional to the rate of growth of the current, and to the self-induction of the circuit. In an alternate dynamo the first definition is meaningless, because there is a field produced by the magnets; so, when the machine is still, there may be a large number of lines of induction through the armature coils without any armature current. If the second definition is taken, there is likewise an electro-motive force produced by the passage through the field, apart from the effect of the armature current.

Take the case of a machine like the Siemens, with coils passing between poles. If there is a continuous current in the armature, in some positions it will assist the field magnets, and

in others it will to some extent weaken them, while in intermediate positions it has no effect on the magnets as a whole. In an alternate-current machine, though the armature current is alternating, relatively to the field magnets it is direct. If a series of instantaneous photographs of the armature current could be taken, each part of the air space would have its direct current.

Fig. 3 is a diagram of the field-magnet poles of a machine. There are supposed, for simplicity, to be twice as many pairs of poles as there are armature coils. If one side of a coil is between each pair of poles, if there

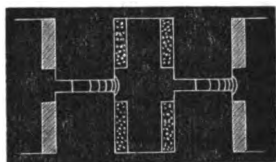


FIG. 3.

were no armature current the fields would be inducing a maximum pressure. If the pressure curve of such a machine

were traced, and placed in the diagram of the poles, it would be as shown in Fig. 4. The positive direction of the current or pressure in the armature and field-magnet coils is shown

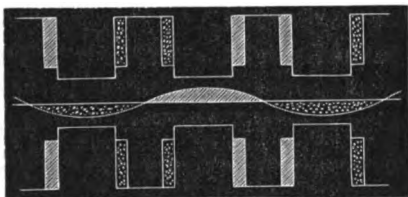


FIG. 4.

by shading, the negative by dotting. If electricity flows in the armature when the sides of the coils are in the active fields, one side of the field between the poles is strengthened and the other weakened, as shown in Fig. 3. This action is fully explained in the case of direct currents in my paper already referred to; but in alternate machines the action is much more complicated, for there is no longer a continuous succession of conductors each carrying the same current as it passes any given position. If the pole-pieces were laminated, the distribution of field would vary as each conductor passed; but as it is not laminated, Foucault currents developed in the pole-pieces tend to check variations under influences varying from 200 to 600 per second. The field thus becomes stronger on one side. The curve of pressure thus gets displaced, as shown in Fig. 5.

If the external resistance is non-inductive, this may be regarded as a current-curve. It will be seen that each magnet is

now exposed to the back induction due to the armature current. A circuit, *a*, taken round the field-magnet circuit is interlinked with the whole wave. The line integral is not changed if it is taken round any other circuit, so that the sides are the same distance apart as they are in *a*.

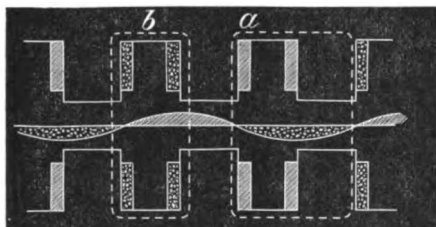


FIG. 5.

A circuit taken as shown at *b*, however, includes only half a wave; so that from Fig. 5 both the cross and the back induction could be completely worked out.

In most machines it is impossible to make such a diagram accurately; but in others, such as drum armatures, with poles and coils of given widths, it is not so complicated.

The effect of the armature current, then, is first to make the current lag relatively to the position of the field magnets, and then to lessen the pressure as a whole. It thus resembles the effect of the insertion of artificial self-induction.

I think to call all this "self-induction," and to assume the harmonic law just to make calculation easy, is disguising the real nature of the action, and may lead to very serious errors. The armature reaction theory is also infinitely easier to understand, and to follow in one's head. The result of it is, in short, that if the current lags, the field is weakened; if it leads, the field is strengthened. If the current is in the direction agreeing with the electro-motive force of the machine, it tends to lag, owing to cross induction; if it is against the electro-motive force of the machine, as in the case of a motor, it tends to cross-magnetise the fields in the opposite direction.

We may now examine the question of parallel running from the armature reaction point of view. Though it may be out of its proper place in the paper, the question of motors may be to some extent discussed at the same time.

Suppose, as in the first case, a pair of terminals in connection with a station, so that the electro-motive force on them is independent of any current, or of the machine under discussion

connected to them. It is necessary to keep clear, for the moment, of any ideas connected with L. The armature circuit must be regarded as devoid of "self-induction."

A machine without friction may be imagined, and when its field magnets are separately excited, so as to give an electro-motive force exactly equal to that of the mains when it is connected so as to be in step, it runs as a motor taking no current. At each instant the electro-motive force of the machine is exactly equal to the electro-motive force of the mains, so no current passes. On putting on some load the machine begins to lag. As soon as the machine lags, a current passes through the armature. The current due to the difference of two equal alternating electro-motive forces is not in step with either of them. This is shown in Fig. 6, where two pressure curves are drawn a little out of step. The current through resistance is proportional to the shaded parts.

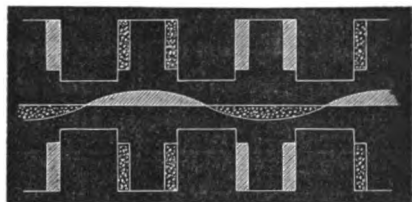


FIG. 6.

If the electro-motive force of the machine were still the same as that of the mains, it would still develop no power as a motor, because at some parts of the period it would be supplying power to the mains, and at other parts the mains would be supplying power to the armature; for at any instant the armature current would be equal to the difference of the two electro-motive forces divided by the resistance of the armature. The average current-curve is shown in Fig. 7. The electro-motive force of the machine does not, however, remain



FIG. 7.

equal to the main electro-motive force, merely differing from it in phase, but is decreased by the reaction of the armature current on the field magnets. The average current-curve shows that there would be considerable back induction, or many ampere-turns tending to weaken the field. This weakening of the field by the armature reaction allows the main also to supply a current against

the electro-motive force of the machine, so that it acts as a motor, and power can be taken from it. If the load is altered, the machine will increase or decrease its lag, so that its effective field is reduced to just the right value to allow the machine to absorb electrically the power taken from it mechanically. As lag is necessary in order to weaken the effective field, so the current in the armature is no longer in step with the electro-motive force of the mains. The result of this is that it is not at the maximum value at the same instants as the electro-motive force is; so that the power taken by the motor is less than the product of the effective electro-motive force of the mains and the effective current of the machine. The electro-motive force and current may be so much out of step that the power of the motor is small, though the armature current is large. The availability of the machine as a motor thus depends on the resistance of the armature, and on its reaction on the field. For instance, if the machine has very little armature resistance, a very small reduction of its electro-motive force will allow a large armature current to pass. If, on the other hand, the armature resistance is high, the armature must have a large reaction on the field so as to reduce the effective electro-motive force. If, at the same time as the load is put on, the field of the motor is artificially weakened by reducing the excitation—for instance, by the insertion of resistance in the field circuit—the electro-motive force of the machine may be reduced enough to allow current from the mains to act in step with the main electro-motive force. For instance, if the supply circuit is of 2,000 volts, and if an ordinary separately excited 2,000-volt machine is connected, and excited so as to give 2,000 volts with the same frequency, and if this machine is loaded as a motor with 40,000 watts, or 54 horse-power, it will lag until the field is weakened enough to let enough current pass to give the power. This current will be more than 20 amperes. But if the machine as a dynamo can give 20 amperes with the loss of 100 volts in the armature, due to the resistance, and if the machine is excited so as to give 1,900 volts on open circuit at the normal speed, and is then thrown into the circuit at an instant when it is in step, and a load of 38,000 watts, or 51 horse-power,

taken out, it will remain in step; for the electro-motive force of the machine is always just enough below the main electro-motive force to allow the right current to pass. The effective current is then 20 amperes. The main circuit thus supplies 2,000 volts and 20 amperes. 2,000 watts are lost in overcoming the resistance of the armature, and 38,000 are available at the pulley. The losses by friction and Foucault currents are here neglected for the sake of simplicity. In practice a little less than 51 horsepower would be obtained for external use. If the field has not been weakened by reducing the excitation, the effective current—that is to say, the root of the mean square of the current, or the current as measured by an electro-dynamometer—would be more than 20 amperes, so that there would be extra loss of power in the armature.

If the machine is to be worked as a generator in parallel with the main circuit—that is to say, if it is to supply power to the mains instead of to take it from them—the same reasoning holds good. If the machine is excited so as to give 2,000 volts at no load, and is thrown into circuit at an instant when it is in step,—if the engine supplies just enough power to overcome friction, there will be no armature current, as the electro-motive force of the machine will, at every instant of the period, be exactly equal and opposite to that of the mains. If the engine supplies just a little too much power, the machine will begin to lead, and an armature current will pass, which will strengthen the fields by reaction, and the machine will at some instants act as a generator, because its electro-motive force is now a little higher, as the armature current has strengthened its field. Similarly, if the steam of the engine were completely shut off, the machine would lag until it worked as a motor. If now the steam is turned on until the dynamo supplies 40,000 watts to the mains, the machine will increase its lead until the armature current strengthens the field enough to give the full output. The armature current must be out of step with the electro-motive force of the machine.

In these examples back induction has alone been considered; but the effect of cross induction must also be taken into account. Cross induction, in the case when the machine is working as a

dynamo, makes the current lag. This weakens the field, and counterbalances to some extent the effect of the leading current, due to the pressures being out of step. A still greater lead is therefore necessary to make the machine work as a dynamo. Suppose, however, that the field excitation is strengthened to balance this back induction, as well as the loss by armature resistance—that is to say, suppose it is excited to give 2,000 volts, and 20 amperes on resistance, in spite of the armature reactions—it will run in step on the mains.

It is thus possible by altering the field excitation of such dynamos to make them work well as generators or motors on supply circuits. Instead of depending on the reaction of the armature to strengthen or weaken the field, the field excitation may be automatically regulated, so that the current and electro-motive force of the machine are always in step, or very nearly so.

Take first the case of a separately excited motor. The field magnets may have an additional set of coils, and these are put in series with the main armature current, which is redressed or rectified by a commutator. These coils are arranged so as to demagnetise or weaken the field as the current increases, so that it is not necessary for the machine to lag in order to take a large load.

The machine may be made to excite itself. In that case, special exciting coils on the main armature are provided. These are connected with the circuit for exciting the field magnet. If a machine made in this way is used as a motor without a series demagnetising circuit round the field magnets, it begins to lag as soon as load is put on, and the armature current then reacts on the fields and weakens them. As the exciting coils now pass through the same field, this will also lessen the field excitation directly; and as soon as the field is weakened by the armature reaction, the ampere-turns round are also reduced, because the electro-motive force in the exciting circuit is lessened. A very small lag will thus weaken the field considerably, so that a large load may be put on without the lag being great enough to make the machine inefficient. A series demagnetising circuit may be used in addition. If the ampere-turns of the field

magnet are not adjusted to the load with perfect accuracy, the machine adjusts itself by lagging or leading very slightly.

Machines with fields excited chiefly by special exciting circuits on the main armatures, and partly by series circuits, may be specially useful for working in parallel as generators. The series circuits are then arranged to magnetise instead of to demagnetise. If power is applied to a motor instead of taken from it, it becomes a generator, and the direction of its main current is reversed, both in the armature and the series coils on the fields; so that a motor with a backward series circuit becomes a generator with a forward series circuit. If one machine when used as a generator begins to lead because too much power is supplied to it by its engine, its field is strengthened by the series circuits, and this increases the armature electro-motive force enough to make it give a large output corresponding to the power supplied to it. If machines excited from special armature circuits, without series circuits, are run in parallel, they will keep in step, because the least leading or lagging alters the field excitation.

The compound form is best, because the whole station will then supply the mains at constant pressure, if all the engines are mechanically governed for constant speed of one of them—that is to say, if the whole steam supply is regulated to keep the speed of all the engines constant, the engines taking the steam in proportion to their size. Instead of keeping the speed constant, an electric governor can keep the electro-motive force of the whole station constant. With alternate machines as at present commonly used, it is necessary to alter the field excitation by hand or by automatic gear. Such machines as those described have advantages corresponding to those of compound direct-current dynamos, compared with shunt or separately excited machines. The analogy between these machines and compound or shunt direct-current machines must not be used too far. In direct-current machines, the electro-motive force adapts itself by slight alterations of speed, and the armature reaction depends on the position of the brushes; in alternate machines, the speed is unalterable, and the armature reaction depends on lead and lag.

Professor E. Thomson tells me that he has been accustomed

to send out separate and series excited machines for working in parallel, and that they will work either with or without "equalising connections" such as are needed with direct compound machines. They also work well as motors, and so do the self-exciting machines with separate exciting armature circuits. He also finds the ordinary Thomson-Houston machine works efficiently as a motor.

In discussing such questions, however, we must always remember that the size of the machine is most important. A small dynamo has high resistance, small cross induction, and small armature reaction. A large machine has a small resistance and great armature reaction. Large machines may therefore easily run well in parallel or as motors, without automatically varied field excitations. A very small lagging current will reduce the field of a large machine 3 or 4 per cent., and the extra loss of power in the armature is small; for if  $C$  is the component of the current that is in step, and  $c$  the lagging component, the power wasted in the armature is not  $r(C + c)^2$ , but  $rC^2 + rc^2$ —a very different matter.

The case of motors, however, is not the same. A two-horse or one-horse motor is about the size that is most needed. There is no opening for 100-kilowatt motors on supply circuits just now. In these small sizes  $c$  will be very large compared with  $C$ , so automatic regulation of the field may be of the highest consequence.

The subject of motors is of enormous importance, and is most interesting. I fear it cannot be pursued here, for want of time. It is to be hoped one of my hearers may be induced to write a paper on this subject at an early date, unless, as is probable, it is exhausted in Mr. Kapp's Cantor Lectures.

According to the armature reaction theory, then, whether a machine will run in parallel with others depends on its armature resistance and armature reactions. It must be borne in mind that most machines have cast-iron fields, and though that may make no difference according to the orthodox theory, it does from the armature reaction point of view. If the field magnets do not quickly respond to the armature reactions, it may be difficult or impossible to run machines in parallel.

Another important consideration is the government of the engines. If two engines are working two machines in parallel at 200 revolutions, and one governor is set for 200 and the other for 195 revolutions,—if the governors are truly isochronous, one engine and dynamo will supply the whole of the power, and the other will be dragged round idle. The whole question of the government of central stations requires the most careful consideration. I have dealt with the matter at some length elsewhere,\* but the subject should be very fully thought out in designing stations.

#### ARRANGEMENT OF STATION.

Whatever system of distribution be used outside the station, the power leaves by high-pressure leads. In this country 2,000 volts is the pressure commonly employed; in America, 1,000. I cannot help thinking that the Americans are right. The use of 2,000 volts does not decrease the cost of copper so very much, and it increases the cost of insulation enormously. In addition to this, it increases the dangers of breakdowns, and the chance of danger to life. 2,000 volts seems to have been chosen in the early days, when people had very little idea of the commercial difficulties of high-pressure insulations. In addition to this, high pressure increases the cost of transformers as well as dynamos, and also lessens the output for a given size.

If the machines are employed in parallel, they may feed into common bars, as in the case of direct currents. If each district must have its own circuit—which seems to be generally assumed necessary—each circuit may be led away from the common bars with a transformer regulator in it. One objection that has been raised against parallel working is that, if each district has its own system, it may have an earth on one lead without disaster, while, if the station is in parallel, two leaks in the whole system cause a breakdown. This objection comes from America, where they have had a great deal of experience, but it seems to assume the leads to be in a very bad state. There is no need for such danger, however, for each district may be fed

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\* *Industries*, September, 1890.

through a transformer, so that it has no metallic connection with the system of any other district. It is most difficult to understand why central station machines are made to give high pressures to begin with. In most forms of dynamos 2,000-volt insulation adds very materially to the cost of the machine, and lessens the output for a given size in addition. The difference of cost would more than cover the extra cost of transformers, as large converters can be made for considerably under £1 per 1,000 watts; and the efficiency, when very large, is very high, being above 99 per cent. at full load. So that the efficiency of a low-pressure dynamo and transformer is higher than that of a high-pressure dynamo working direct. It is also a great advantage to have all the switches, connections, and measuring instruments on the low-pressure side. The high-pressure circuits may then be arranged to have no metal exposed anywhere. If each district must have its separate circuit, it has its own step-up transformer, with its regulator. If feeders are used on any system, each feeder has its step-up transformer, which is made adjustable; or it may have an unadjustable transformer and a feeder regulator such as that designed by Mr. Kapp. If the district is large, it may have large transformers. Several feeders may then come off in parallel, each feeder having its own regulator. The regulators may have the adjustable parts connected with the low-pressure supply, so as to avoid high-pressure switches or other gear.

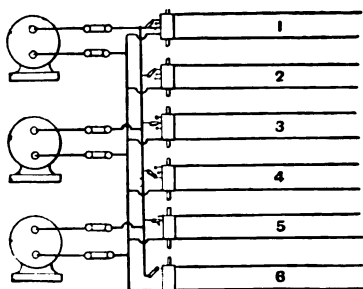


FIG. 8.

One arrangement is shown in Fig. 8. The dynamos are all in parallel, and wound for 100 volts, the immediate neighbourhood of the station being fed direct. Each feeder circuit has a transformer with a variable number of primary turns. A compensated voltmeter is placed on each, and all the attendant

has to do is to alter the switches a little occasionally, so as to keep all the instruments' pointers vertical.

## TRANSFORMERS.

It is not my intention to go into the question of leads, as I am informed that a paper specially devoted to that important subject is to be read here very shortly; I am therefore most happy to leave the very special question of high-pressure alternate-current mains in far abler hands.

Before discussing the relative merits of house transformers, low pressure direct from the station, and various sub-station arrangements for distribution, it will be best to consider the vexed question of the transformers themselves.

In 1884 electrical engineers were surprised to learn from Dr. Hopkinson that the Gaulard & Gibbs transformers gave something like 90 per cent. full-load efficiency. Since then alternating work has developed enormously; but a complete change has come over the transformers: they are almost always made with the iron circuit closed on itself. The closed iron circuit has been regarded as an enormous advance on the old form; but it is difficult to find any definite statement as to where the advantage comes in. It was for a long time assumed that almost any closed-circuit transformer would give an efficiency of something like 98 per cent.—in fact, that the loss in transformers might be neglected.

In a discussion on the relative merits of direct- and alternate-current distribution, Mr. Crompton said that the efficiency of the ordinary commercial transformer in a station was about 50 per cent.; and I need hardly say he was not believed. He laid particular stress on the small proportion of the total number of lamps installed really used. The loss in the iron in transformers has until lately been very much overlooked; if it is even a small percentage of the full load of the transformer, it mounts up very seriously in station work when the transformers are in circuit all day and all night.

I had the honour of reading a paper before the British Association in 1889, in which the question of design for all-day loads was, as far as I know, discussed fully for the first time; and the relative proportions of iron and copper for closed-circuit

transformers which gave the best results were given. At the same time I pointed out that the loss in iron was really such a serious matter that by a special design, not only can a very much higher efficiency be obtained at light loads, but that even at full load a new form is better in every way.

The principle of the "Hedgehog" transformer is this: By using an open circuit the iron can be reduced in volume with a given induction. The mean length of the iron part of the magnetic circuit is considerably less, and more copper turns can be wound on without increasing this length much. The additional turns admit of a smaller cross section of core, so that the iron is enormously reduced. The loss in copper is somewhat increased; but, as the loss in copper is only serious at full load, this does not matter.

The old Gaulard & Gibbs form of transformer had the disadvantage of requiring a very large exciting current. Increased number of turns of copper reduces the exciting current; but, in addition, the ends of the core are spread out in a peculiar way, which again reduces the exciting current. The result is that the exciting current, though larger than in the case of closed-circuit transformers, is not a serious matter. The loss in copper is considerably smaller than the loss in iron in most closed-circuit transformers, and, as it varies as the square of the current, it is inappreciable except at full load, which very seldom occurs. The loss in iron is very small, so that the loss per day in ordinary working is very much smaller than in the case of closed-circuit transformers. Since my paper at the British Association, many makers have altered the proportion of their transformers considerably; but still it is impossible to get as good results with closed circuits.

Perhaps the best way I can make this clear is by a comparison of actual transformers. I am here confronted by a difficulty. If I select any particular closed-circuit transformer of a particular maker, all the other makers of closed-circuit transformers would say that I had chosen a bad example; while the maker himself would say that it was an old form, or that he now uses a quality of iron which wastes no power by hysteresis.

A great deal of unnecessary mystery has been made about the efficiency of transformers, and all sorts of roundabout ways of measuring or calculating it have been proposed, and sometimes tried. The matter is simple, however. The loss in the copper can be calculated if the resistance and current are known. The loss in iron in a given type of transformer depends on the quality of the sample used just as much as the loss in the copper depends on the quality of the copper used. I will therefore assume the ordinary "96 per cent." quality of copper, and I will assume that the iron is as good as the soft iron tested by Professor Ewing. I have the best reasons for knowing that the iron used is generally much below this standard, so this assumption is considerably in favour of the closed circuits. Moreover, I am giving away the loss by Foucault currents, which is often considerable. Of course, anyone is at liberty to assume that he has some particularly good iron; but in discussing the relative value of the two types of transformer, he must allow me the same quality of iron. In fact, I might assume a special quality of copper equally unknown to the trade in mine.

As an average size of transformer, one taking 2,000 volts and a frequency of 100, and giving 15 amperes at 100 volts, may be taken, and the working compared.

Though the theory of the "Hedgehog" transformer was fully discussed in the British Association paper referred to, it may not be out of place to describe the construction of the commercial article.

A gun-metal casting forms the backbone of the transformer. It is cross shape in section, and at one end is spread out to form four legs. It also supports the lower flange, or cheek, of the winding. The four spaces have bundles of soft iron wire put in so that the core is practically cylindrical. The cross-shaped core has been criticised on the ground that there are Foucault currents in it. The least consideration will show that there is no change of induction in it, and therefore no Foucault currents. The iron wire is bound tightly, and the two flanges are slipped into place. A gun-metal "spider" is then screwed to the top. It is finally held on by the eye-belt, but temporarily a set screw is used. The

core is then wound in a special lathe in the usual way. Both the terminal board and the coil flanges gave us a great deal of trouble till we found suitable materials. There is a great need of good insulating materials that will not warp, that will insulate perfectly, and that can be worked.

The electrical particulars of a 50-8-candle-power lamp transformer are as follows:—

Core, 13.5 in. long, 2.5 in. diameter. In taking the area of the core, allowance must be made for the space occupied by the backbone, and for the loss of room in using round wires. Maximum induction, 4,800. Loss of power in iron, assuming it to be of good quality, 13.5 watts. Primary, 3,920 turns of copper wire, 0.042 in. diameter; resistance, 37 ohms (warm). Secondary, 200 turns,  $19/0.042$  in.; resistance, 0.098 ohm. Loss of power in primary at full load, due to the transformed current, 21 watts; loss due to the exciting current — 0.3 ampere — which is always on, 3.6 watts; loss in secondary at full load, 22 watts. Efficiency at full load, 96.15.

This transformer is shown to scale in Figs. 9 and 10.

Fig. 11 is a scale drawing of a closed-circuit transformer I have taken as representative. The primary is 400 turns of 0.042-in. wire, and the secondary 20 turns of 0.176-in. The loss in copper at full load is 11.3 watts, and the loss in the iron 120 watts.

At full load the efficiency is 91.95, or nearly 92 per cent. The diagram (Fig. 12) shows the percentage loss at various



FIG. 9.

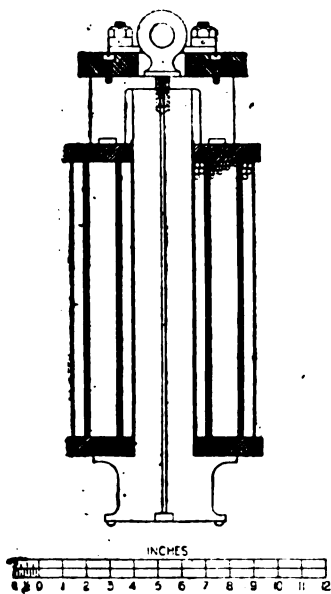


FIG. 10.

loads of the two transformers. The ordinates are percentage loss, and the abscissæ fractions of full load. Curve II. is the



FIG. 11.

"Hedgehog," and IV. is the closed circuit. It will be noticed that the "Hedgehog's" efficiency increases slightly at first, and keeps above 95 per cent. till the load is reduced to one-fifth, and above 90 down to one-tenth load, and it then begins to fall more rapidly. The efficiency of the closed form, however, begins to fall at once, and falls rapidly. In house work the full-load efficiency is a matter of little or no consequence; the loss at very light loads is really important. A house that has a 50-light transformer is wired for 50 lamps, and that is the maximum load. As a rule, there are seldom more than 25 lamps on at any one time, and these are on during the evening only. The average load varies with the time of year, and with the class of house. Shops, for instance, have full load for a short time each day; private houses have the lights in the hall and passages on the whole evening, with either the dining or drawing room, and for some time both the dining and drawing rooms are on. It will be a fair average if we take four hours of half-load per day as the output of house transformers. This means an efficiency of 83.3 per cent. for the "Hedgehog," and only 51 per cent. for the closed circuit. In other words, the waste in the closed circuit is between five and six times as great as in the open circuit.

It will, I have no doubt, be urged that various published tests have shown closed-circuit transformers to have efficiencies of, say, 95 per cent. I have elsewhere\* examined some of these tests, and shown that, according to the figures given, the results can only be accounted for by the iron acting as freezing mixture, and supplying power to the transformer; the total loss given being

\* "Induction, and other Things," *Electrical Review*, October, 1889.

in some cases less than the loss by copper resistance. In some other cases too few data are given, so that it is impossible to say how the figures are obtained. In order that an efficiency test can be taken as reliable it must contain full particulars of the transformer. It is then possible to see whether the supposed efficiency of 95 per cent. is due to the type of transformer, to inaccuracies in the measurements, or to phenomenal iron. Of course it is not impossible some one may discover phenomenal iron which will be the salvation of closed-circuit transformers, but it will

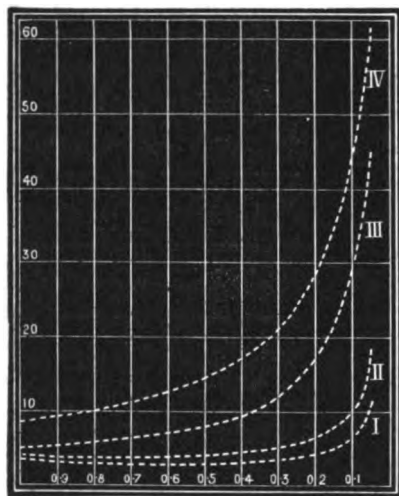


FIG. 12.

have to be very good. Curve III. represents the loss in a closed-circuit transformer with phenomenal iron having a loss of only 60 watts, or half that taken in the example. The dotted curve (I.) belongs to the open-circuit transformer with the same quality of iron; for, of course, the phenomenal iron is available for both. It will be seen that the ratio of the percentage losses is sensibly the same, so that the open-circuit transformer is

improved nearly as much as the closed. In the case taken of half-load for four hours a day, the loss in the closed circuit comes out more than four times that in the open, even without re-designing the "Hedgehog." There is thus no chance of iron being found good enough to allow the closed circuit-form to compete with the open. There is one objection to the "Hedgehog" which must not be passed over, and that is the exciting current. The exciting current in the 50-lighter is 0.31; in the closed circuit it is 0.2; so that it is quite half as much again. Moreover, if a better quality of iron is discovered, it will lessen the exciting current in the closed circuit, but will make no perceptible difference in the open, unless it is re-designed. The

exciting current does harm in two ways: it weakens the field of the dynamos at the central stations, and it gives rise to troubles in the station at light loads. For instance, if a station had 10 dynamos to run the full load, in the daytime three would be required to supply the exciting current, while in the closed circuit two would do. If the dynamos are direct-driven, this means three engines running light instead of two; and engines running light are very wasteful. These difficulties are, however, to a large extent imaginary. The weakening of the fields is, in ordinary working, counteracted by stronger excitation. Running three dynamos instead of two or one does not involve running three engines: the engine should be uncoupled and the dynamos run as idle motors, supplying the exciting current necessary for the transformers. In the future, it is to be hoped that alternate motors will be used; and as they will be used chiefly during the times the load on the transformers is light, they will thus supply the exciting currents, and it will be unnecessary to run any idle dynamos. It must be remembered that the difference is only one of degree. Closed-circuit transformers have very considerable exciting currents; but it is most desirable to avoid any troubles, whichever kind is used. The solution of the difficulty lies in the employment of condensers. A condenser supplies the exciting current, so that the station dynamos supply a current sensibly proportional to the power used. Thus, in the daytime, with open-circuit transformers, a very small engine and dynamo will give all that is needed. More will be said about condensers presently.

### DISTRIBUTION.

Even with the most efficient transformers, the house system is a very poor method of distribution. Apart from the question of loss of power, consider the outlay involved in providing each consumer with a high-pressure double-pole fuse, a transformer, an automatic device for rendering the whole arrangement safe in case of insulation giving way, perhaps a double-pole main switch, a meter, and a fire-proof safe in which all these things have to be enclosed. In addition to this, it involves a complete high-

pressure parallel system of distribution, and a network of 2,000-volt underground mains, involving a heavy outlay, and probably heavy depreciation. The adoption of sub-stations is generally regarded as the cure for all this. I believe no sub-station distribution is in use, or even in construction, in this country. The London Electric Supply Company are using sub-stations, but not in this way. These sub-stations merely transform from 10,000 to 2,500 volts, and the distribution itself is carried out by means of house transformers. The principle of sub-stations is that the town has a network of low-pressure mains, with low-pressure feeders led to sub-stations. In each sub-station there are attendants who regulate the pressure in the network, as they have control of the feeders. The sub-station is supplied by high-pressure mains from the station, the power being transformed to low pressure at the sub-stations. There is, however, no need for sub-stations; the low-pressure network may be fed at various points by transformers and feeders coming direct from the station. These transformers need not be in houses or cellars; they can be placed conveniently in the ground in cases filled with insulating oil. The method of feeding into a low-pressure network was described in Mr. Kapp's paper on transformers in 1888, and he referred to it as being in use on the Continent and in America. At the station each feeder can be regulated, and has a compound-wound voltmeter, so that an attendant can keep the pressure in the whole town constant. As Mr. Kapp pointed out, the advantage of such a system is enormous. In direct currents the low-pressure mains must be large. The great size needed for parallel distribution was pointed out at the time of the Electric Light Act of 1882, and Professor Forbes afterwards went into the matter much more fully; but even now the enormous saving that can be effected by using small low-pressure mains fed at frequent intervals seems hardly to be realised.

In beginning in a new district it may be objected that the subscribers at first are so few that it does not pay to put down low-pressure mains. This would be true if alternate low-pressure had to be as large as direct-current mains. Without going into the cost of a particular case, it is probable that low-pressure mains

large enough to supply any demand likely to arise for many years can be put down for little, if any, more than the high-pressure network with its expensive insulation. Even in scattered districts it would generally pay to use small low-pressure mains and feeders. The alternate system has so many advantages over the direct in the matters of regulation and distribution, when worked on the low-pressure system, that it is astonishing that such arrangements are not utilised. For instance, with direct currents, it is most difficult to find a leak; and one leak on the positive at the west end, combined with one leak on the negative at the east, will cause serious trouble. With alternate currents, a number of small local networks may be used, each of them being complete in itself. It is not in metallic connection with any other part of the town, nor with the central station. Where it meets the network of the neighbouring district it is not metallicity joined, as in the case of direct currents, but is connected through a one-to-one transformer sunk in the ground, in a case full of oil.

After the clear and able manner in which Mr. Kapp urged the advantages of low-pressure networks for alternating currents, it is most astonishing that engineers should go on with house transformers. With the low-pressure network system we have everything that can be wanted. Perfect regulation at every point throughout the town, small mains, inexpensive insulation, cheap and efficient plant, and—what is of importance in comparison with battery work—simple and inexpensive switch-boards. There is no trouble from general ground leak; and both arcs and incandescent lighting can be done from the same leads. The only thing wanting is storage; and to what extent is that really necessary, and what does it cost? It is significant that throughout America, where direct-current stations could have it for the asking, it is never used.

At present, however, we are using alternating currents in just about the worst way that could be possibly devised.

#### CONDENSERS.

Condensers frequently figure in electric light literature, but they are only put forward as possible means for transforming

currents. Direct currents and interruptors are generally discussed. Condensers, may, however, have another very large opening in connection with ordinary alternate-current distribution. Take, for instance, the case of arc lamps. It is customary, whenever possible, to run arc lamps in long series with high pressures, generally with direct currents. In many cases such an arrangement is very convenient; but wherever there are incandescent lamps to be fed at the same places as the arcs, it is, of course, wasteful to run two sets of engines, dynamos, and leads. With direct currents this is sometimes necessary, for arc lamps in parallel do not burn well on 100-volt circuits, and must be used two at a time; and at 50 volts resistance is needed in series, which is wasteful. With alternate currents nothing is easier than to run lamps with choking coils. If the supply is high-pressure, each lamp can have its own transformer, and the transformer can be wound for nearly constant secondary current. Professor E. Thomson has brought out several forms of constant-current transformer, and so has Mr. Tesla. The "Hedgehog" form, however, lends itself especially well: all that is necessary is winding the coils one at each end instead of one over the other. Whether the mains are high or low pressure, choking coils or constant-current transformers will give rise to large currents at the station, which will lower the output of the machines.

For instance, suppose a station supplies a 100-volt secondary circuit by means of transformers with 2,000 volts primary pressure; and suppose there are 200 arc lamps, each taking 33 volts and 10 amperes, arranged on the 100-volt circuit, with a choking coil in series with each. Suppose, for simplicity, that there are no incandescent lamps in use. The arc lamps then take 2,000 amperes and 100 volts; but this does not amount to more than 66,000 watts, as the current is not in step with the electro-motive force. The dynamos thus have to supply 2,000 volts and 100 amperes; whereas the same power, if the current did not lag, would be supplied by 2,000 volts and 33 amperes. This gives rise to several troubles. Suppose each machine has an armature wound to give 2,000 volts and 33 amperes, or 66,000 watts, it takes three instead of one to work the lamps.

In addition to this, the lagging of the current weakens the field of the dynamos, so that they need more excitation to give 2,000 volts than they would need even on full load on a non-inductive resistance. Moreover, running three engines and dynamos at a third load instead of one at full load is not economical. A condenser takes a current which leads relatively to the pressure, so that it counteracts the effect of the lagging current. The 100 amperes may be regarded as compounded of 33 amperes in step with the pressure, and 95 amperes lagging a quarter of a period. A condenser of about 85 microfarads will supply the 95-ampere component, while one dynamo supplies the 33 amperes really needed by the lamps. This reasoning holds good without the curve of sines assumption.

The use of condensers to overcome troubles arising from exciting or magnetising currents of closed or open circuit transformers has already been mentioned.

They may also be used to increase the output of dynamos. As already explained, even if a dynamo is worked on resistance, the cross induction causes the current to lag, and the back induction weakens the field. If a condenser is put in shunt to the terminals of the machine, its current strengthens the field again and increases the terminal pressure, and therefore the output. If the condenser is large enough, and if the armature actions are great enough, it might be possible to make a machine excite itself without any field winding at all. This action of a condenser on a dynamo may be equally easily explained on the self-induction theory. The difference between the two theories may be shown here, however. According to the self-induction theory, if the condenser is large enough to produce resonance—that is to say, when  $(2 \pi n)^2 L K = 1$ —the output increases till the current is as great as if the no-load pressure of the machine were short-circuited on the armature resistance. According to the armature reaction theory, the machine increases its field till the field magnets become more saturated; in fact, the machine is like a direct-current shunt-wound dynamo which has a low-resistance shunt. I do not wish to go into this matter further here, for it might lead to a discussion of a rise in the Deptford mains—

a subject which will be treated, I am informed, in a forthcoming paper, and will certainly receive full justice at the hands of the authors.

The commercial manufacture of high-pressure condensers is not so easy as might be supposed. Knowing the difficulties there are in insulating leads and mains—in which thick insulation is permissible, and, in fact, advisable—it may be realised that to insulate many hundreds of square feet of metallic surface with the thinnest possible material is anything but an easy problem. In addition to being a good insulator, the dielectric must be free from “absorption.” Some account of my work on condensers has lately been given.\* Mr. W. F. Bourne, who has carried out all the experiments, has tried almost every conceivable kind of dielectric, and finds a kind of paper called “butter-skin,” soaked in paraffin oil, the best material. It is curious that this particular kind of paper is specially made so as to be grease-proof. Some kinds of bank post paper worked nearly as well, but it is too expensive for commercial use. It seems as if the paper must be relied upon for insulation, not the paraffin. The condensers are simple cast-iron boxes containing numbers of sheets of paper and tinfoil. Lids are fitted on and made tight.

#### TOWN LIGHTING.

Alternate arc lamps have already been discussed to some extent. In some places, especially in small towns, it is usual to have special circuits for the town lighting, which are shut off during the day. It is needless to point out that if the low-pressure network is used, condensers at the station allow arc lamps to be run with choking coils, and incandescent lamps can be run direct, and it is no more necessary to run special mains for the street lighting than it is needful to put down two sets of gas pipes in a town. Sometimes, however, high-pressure mains are used. Each arc lamp can then have its own constant-current transformer. If incandescent lamps are used, the plan generally adopted is using a transformer for every 10 or 20 lamps. If the wires are underground, this involves great expense; and if overhead, it means both expense and unsightliness. A small

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\* *Phil. Mag.*, February, 1891.

"Hedgehog" transformer has been designed to meet this particular want. It is so small that it can be worked into the design of the lamp-holder or shade, and one can be used for each lamp.

In America alternating arc lamps are run in series, with a series transformer for each. This arrangement is quite unnecessary for places where there is incandescent lighting too. It has the advantage of allowing series arcs without bringing high pressures into the lamps. It has no other advantage over direct series lighting. The dynamo used by the Westinghouse Company is made to give nearly constant current. This is a barbarous arrangement. It is a reversion to the old alternating machines of 10 years ago, which gave approximately constant current. All that is needed is great armature reaction, and hence a large machine for the output. The economical way of getting constant current is to use a good dynamo, and to take the governor off the engine, or to use it only as a safeguard against racing. If the Westinghouse machine has only quarter load on, it still goes at full speed, using nearly full steam, and wearing everything out. With a constant-pressure dynamo it would run at quarter speed, each cylinderful of steam being used economically and being expanded properly. To put a constant-speed governor on the engine, and then to design a special dynamo to get over the difficulties you have introduced, is a very common proceeding with direct-current arcs also.

Alternating constant currents have, however, one advantage over direct that I think is not realised. Synchronising motors will run perfectly on series constant-current circuits; and as they run at constant speed, there is no racing, or trouble about governing, as in the case of direct currents. Of course, constant speed of engine is here necessary to give constant frequency. Many people seem to have gathered from Dr. Hopkinson's paper that series alternating motors will not run; but this is a false inference.

The PRESIDENT: The applause which has just greeted the speaker tells me that it is almost unnecessary to propose a vote of thanks for his paper. I see around me a number of gentlemen

anxious to take part in the discussion which will follow, but it is too late to commence this evening, and we will therefore take the discussion on this paper at the next meeting, which will be held on February 26th. I will now only ask you to give a hearty vote of thanks to the author.

The vote was unanimously accorded.

A ballot for new members took place, at which the following candidates were elected :—

*Foreign Members :*

Felix Deutsch. | Arthur Jourdan. | H. C. Kragh.

*Member :*

William Arnot.

*Associates :*

Edgar A. Ashcroft.  
Albert Thomas Bartlett.  
Frank Whinfield Cawter.  
R. H. T. Drummond.  
Randall Howard Fletcher.  
Norman Julius Hockley.  
Basil Fenwick Howard.  
Alfred James Jarman.

James Oddie.  
Theodor Petersen.  
William Robert Rawlings.  
Sidney Stokes.  
Samuel Probart Street.  
Sydney Bousfield Tatham.  
Leicester William Woodman.

*Students :*

Henry Melville Ackery.  
Henry Mathew Alleyn.  
H. James Bubb.  
Cecil Buchanan Clay.  
James Ernest Francis Collum,  
B.A.  
John Corbett.  
Charles Wright Durnford.  
Joseph Thompson Elliott.  
Herbert Anthony Evans.  
Bertram Annandale Giuseppe.  
Herbert Justice Glynn.  
Walter Cluny Stewart Holland.

Arthur Llewelyn Lean.  
John Leggat.  
Eustace Ronald Maples.  
Percy Walter McDougall.  
Gustavus Frederick Moller.  
Henry Harold Perry.  
Harold H. Simmons.  
Hubert Conrad Sparks.  
Edward Thorold Stewart-  
Menteath.  
Morris Charles Tiarks.  
Walter Adolph Vignoles.  
Frederick Young.

The meeting then adjourned.

The Two Hundred and Eighteenth Ordinary General Meeting of the Institution was held at the Institution of Civil Engineers, 25, Great George Street, Westminster, on Thursday evening, February 26th, 1891—Professor W. CROOKES, F.R.S., President, in the chair.

The minutes of the ordinary general meeting held on February 19th, 1891, were read and approved.

The names of new candidates for election into the Institution were announced and ordered to be suspended.

The PRESIDENT: Before the discussion is commenced on Mr. Swinburne's paper on "Transformer Distribution," he would like to add a few words.

Mr. SWINBURNE: I would like to call the attention of the meeting to the transformers which are on the table. One is for 40 50-watt lamps at 1,000 and 50 volts for a frequency of 130, and is a sample of our American type. The next is for 100 30-watt lamps at 2,000 and 100 volts and 80 periods, for France. The third is a street lighter for a 32-candle-power lamp. The transformer, which is 3 inches in diameter, is worked into the design of the lamp shade. Mr. Swinburne.

I expect a great deal of discussion as to transformers, and trust that any accounts of efficiency tests will be accompanied by full particulars of the transformer, so that we may know what it really means. I have given you full particulars of my own transformer, and hope others will treat the Institution equally fairly.

Mr. W. B. ESSON: The paper read by Mr. Swinburne last week will be considered a most important one, as serving to rectify certain errors which have recently crept into the considerations of some regarding the action of alternating-current machines. The paper of Mr. Mordey, and the discussion thereon, in 1889, left things in a most unsatisfactory condition. The former, it will be remembered, gave no figures, and we were expected to take it on trust that the experiments were capable of complete explanation on the lines of the author's theory. Mr. Eason.

Mr. Eeson. without, however, having any data furnished which would enable us to confirm or confute the theory then received. The discussion showed that Mr. Mordey's adherents were in the minority, and when his reply at length appeared in print, it was gratifying to find in the very last sentence a clear indication of the author's conversion. He affirms there his complete agreement with a speaker who said, "On the other hand, it is equally true that "some self-induction must be present, otherwise no controlling "couple could exist."

Mr. Mordey, then, eventually admitted that there must be *some* self-induction for proper control in parallel working, but up to the date of his paper no one seemed to have a very clear idea of how much. Mr. Swinburne seemingly had fallen into the same error as a good many more of us, in supposing that coreless armatures had not sufficient self-induction to enable them to run in parallel, and, to supply the deficiency, he at one time proposed the use of an induction coupler. Mr. Mordey, on the other hand, thought that because his armature had no core it had practically no self-induction. These two ideas were a few years ago shared by everyone. Now we know that they are both wrong; that a coreless armature may have self-induction sufficient to make running in parallel quite easy, and that this self-induction may be comparable with that of an iron-cored armature.

It appears that the classification of machines into those having iron in their armatures and those without has been productive of a good deal of misconception. So far as self-induction is concerned, there is really very little difference between a modern Siemens or Ferranti alternator and one constructed on the Lowrie-Parker or Westinghouse lines. Both have about the same length of gap and the same induction in the gap; the same effect is produced on the field by the coils moving across it; and the difference between the two exists only in the fact that in the cored armature some part of the iron constituting the magnetic circuit revolves, and is utilised to lay the generating coils on. The following diagrams show this quite clearly, Fig. 1 showing part of a coreless armature and its magnets, and Fig. 2 a cored

armature. The gaps are interposed at a different part of the magnetic circuit, and the core in Fig. 2 takes the place

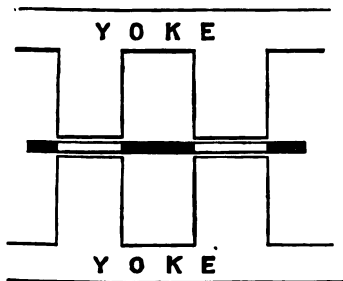


FIG. 1.

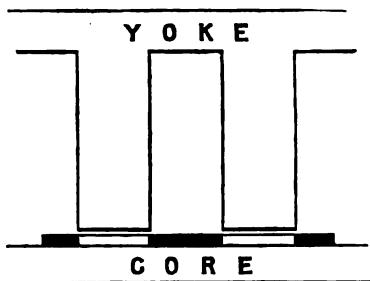


FIG. 2.

of one of the yokes in Fig. 1. The armatures of all are coreless, in the sense that inside the coil itself there is no iron; and the distinguishing feature of the cored armature, then, is not that it possesses greater self-induction than the other, but that part of the iron forming the magnetic circuit moves. I believe I am correct in assuming that Mr. Swinburne puts forward his explanation of parallel running according to the armature reaction theory, as in every way compatible with Dr. Hopkinson's theory. On referring to the discussion on Dr. Hopkinson's paper, I find that in the term "self-induction" he included the armature reactions; indeed, it is difficult to imagine how we could deal with self-induction as exhibited by alternators without including the reactions. The effect made apparent in the lagging of the current and diminished pressure as a whole, is simply the result of superposing on the magnet field another field due to the alternating current in the armature; and any term, call it "self-induction" or what you like, introduced for the purpose of expressing the armature effect must take notice of the several reactions Mr. Swinburne mentions. On the basis that all these may be treated as self-induction—*i.e.*, as producing an E.M.F. in quadrature with the C.R. or resultant E.M.F.—Dr. Hopkinson has told us what should be its value; but though Mr. Swinburne objects to such treatment on the ground that the harmonic law does not hold, I do not find in the paper any hint as to how the amount of the reaction effect is to be estimated, or any indication of a

Mr. Eason. law that does hold. Though we are all convinced at this stage that neither the forward nor back E.M.F.'s are strictly sine functions of the time, it does not appear that any great error is made by assuming them to be so; and if this assumption is not made, our diagrams become, like Dr. Hopkinson's formulæ, unmanageable. We may be able from a knowledge of previous machines to make a guess regarding the effect, but if calculation is to be attempted, it must be assumed that the harmonic law holds. When we interpret "self-induction" in its wider meaning as embracing all the reactions due to the armature current, its influence in controlling machines in parallel becomes perfectly clear. It is no fault of theory if some have misunderstood it by interpreting "self-induction" in too narrow a sense, and the best thanks of all are due to Mr. Swinburne for putting the whole matter so clearly.

With Mr. Swinburne's objections to a complicated network of high-pressure mains I heartily agree. In the discussion on Mr. Crompton's paper, three years ago, I made some observations similar to those Mr. Swinburne now makes regarding the risk with house-to-house transformers, and I have seen no reason to alter my opinion. I believe, moreover, with the author of the paper, that the Americans are right in using 1,000 volts in place of our 2,000. But with a properly arranged switch-board there need be no hitch at the station in the case of an engine breakdown, and I do not quite see the advantage of feeding the parallel machines into common bars and then isolating the supply circuits by feeding them through transformers. It seems very much simpler to dispense with the common bars altogether and feed into the supply circuits direct, unless, of course, it is desired to feed through a step-up transformer. But however this is arranged, there is no doubt that for alternating currents, low-pressure distribution with high-pressure feeders to transformers at different points in the system will eventually be the rule.

Coming to the particular design of transformer which Mr. Swinburne recommends, I think it will be admitted by all that the considerations which have weighed with the author in

designing this particular type are of the greatest importance. Mr. KESOU. It is only misleading to say that the efficiency of a transformer is 90 per cent. at half-load if the secondary is closed with this load for only four hours per diem and no notice is taken of the energy wasted in reversing the magnetism of the core for the remaining 20 hours. Taking an average day, the true efficiency must be the ratio which the energy appearing at the terminals of the secondary circuit during the hours of lighting bears to the energy expended in the primary during 24 hours, and it is upon this ratio that the efficiency of the transformer system mainly depends. But let us see if Mr. Swinburne's calculation is quite correct for the closed-circuit transformer. Taking a 50-8-candle-power one tested by Professor Ayrton some time since, and which had an efficiency of 90 per cent. at half-load—we find that when the secondary circuit was open 53·18 watts were expended in heating the primary and the iron. We had then  $53\cdot18 \times 20$ , or 1,063·6 watt-hours, to be debited against it for the 20 idle hours. During the time it was working at half-load—750 watts—we had 833 watts given to it, which puts another  $833 \times 4$ , or 3,332 watt-hours, on the debit side. The total against the transformer is 4,395 watt-hours, giving it the benefit of the fractions, and the credit is  $750 \times 4$ , or 3,000 watt-hours. This shows a 24-hour efficiency of nearly 69 per cent., which is not high, but which is considerably greater than the 51 per cent. which Mr. Swinburne gives for one of the same size, and was obtained, not yesterday, but more than three years ago, when designers were not so keenly alive to the great importance of diminishing the iron losses. Of course I am relying upon Professor Ayrton's figures; and Mr. Swinburne having discredited the closed-circuit type, it is quite possible that he may have exaggerated to a corresponding extent the merits of the open-circuit type.

I am glad Mr. Swinburne has brought his "Hedgehog" to the Institution, because it is important that its nature and habits be thoroughly investigated. We are all agreed that it is of the greatest importance to make the all-day efficiency as high as we can, but it is doubtful whether many of us will agree that Mr.

Mr. Eason. Swinburne takes the best way of doing this. As a matter of fact, one cannot check the figures he gives at all. If we make his own

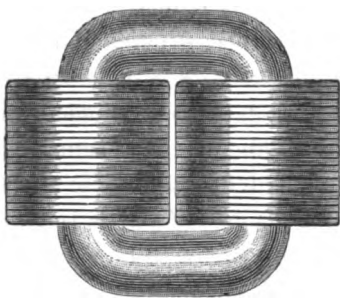


FIG. 3.

transformer a closed-circuit one by cutting the "Hedgehog" in two and placing the halves side by side, as in Fig. 3, we increase the watts spent in the iron by 66 per cent. at the very utmost, which gives us for the closed-circuit type 22·5 watts, instead of 120, as Mr. Swinburne makes it.

I do not know how the latter figure is arrived at, as I have not, while writing, Mr. Swinburne's diagrams by me; but if 13·5 watts are correct for the open-circuit type, 22·5 would be correct for the closed-circuit; and that the author has made a mistake in the figure 120 watts is certain, for in the transformer to which I have already referred, designed three years ago, the whole loss on open circuit was only 53·18 watts. If a correction is made for this error, it will be found that the all-day efficiency of the closed-circuit transformer (Fig. 3) comes up to 80 per cent., and by re-designing it might be made higher still. It is by no means clear, therefore, that the "Hedgehog" is more efficient than ordinary types, while it labours under the great disadvantage of requiring a large exciting current. Probably, when both are well designed, there is little to choose between them.

In 1885, some time after my firm had constructed for the National Company those wonderful arrangements of parallel columns used at Edgware Road—as complex in their regulating gear as a machine gun—I told Gaulard & Gibbs to join the cores of these secondary generator columns at the top and complete the magnetic circuit through iron. They acted upon the suggestion, and a transformer resembling Fig. 3 was the result. It is really a closed ring of iron—a Gramme ring—and in a letter published some time after, Mr. Blathy mentioned that experiments with this kind of transformer were also in progress at Ganz's works as early as August, 1884. Mr. Kennedy claims to have used a Gramme

ring in 1883; and Ganz's engineers, after trying several types, Mr. Eason. have returned to it. It is evident that this form, when properly proportioned, is hard to beat, and I do not recognise in the numerous designs of the present day any considerable advance on this particular shape suggested by me six years ago.

Mr. MORDEY: I think, Sir, that every paragraph in this very Mr. Mordey. interesting paper could be usefully discussed, but perhaps I can best use the time at my disposal by confining myself mainly to one section of the paper. I will therefore pass over the first part of Mr. Swinburne's paper, where he treats of armature reactions and parallel running. I gave a practical contribution to the question of alternate-current dynamos a couple of years ago, and I have very little to add to what I said then. I think the real question to discuss is not so much armature reactions as a matter of theory, as transformer design as a matter of practice. I will, therefore, with your permission, confine myself almost entirely to the question of transformers; but, in passing, I may say that I agree with the author on many of his points in connection with distribution, and congratulate him very heartily on the progress he appears to have made in the construction of condensers, which I shall be interested to see in use on high-tension circuits.

I am glad that Mr. Swinburne has given us an opportunity of discussing his views on transformers, which he first expressed in 1889 in his British Association paper. When a practical man reads a paper at the British Association, it may be that he does so because he does not wish it to be discussed. But now that the author has fully matured his views, and given them concrete expression, it is right that this Institution should examine them. Developments are so rapid in these matters that it will occasion no surprise to find that Mr. Swinburne three years ago was not in favour of open-circuit transformers. I find in the *Journal* for 1889 (six months before his British Association paper) that, in a discussion on a paper by Professor Forbes, Mr. Swinburne said: "I notice a slip where Professor Forbes said that some transformers would be more efficient if the iron circuit were open. As I fell into the same

Mr. Morley. "error myself, I may point out that the waste of energy is  $\int I dH$ ;" and so on.\*

As some of us have gone on making closed-circuit transformers, just as if Mr. Swinburne had not proved to us in his British Association paper (and on paper) that we were all wrong, we must justify or explain our action as well as we can.

In order that I may not base my remarks on any misconception, I will first state Mr. Swinburne's views regarding transformers; then I will refer to some tests I have made, and then examine Mr. Swinburne's views in the light of these tests, as I think we want now rather tests than calculations or theories. When we have the facts we can usefully theorise.

He condemns closed-magnetic-circuit transformers, and bases his preference for open-circuit transformers on the following assumptions:—

- (1.) That loss from hysteresis is heavy and constant for all loads.
- (2.) That the losses are easily ascertainable by calculation only—from Ewing's or Hopkinson's determinations for the hysteresis, from  $C^2 R$  in the conductor, and by a more elaborate calculation for the eddies.
- (3.) That, the action of the iron and the other actions in transformers being fully understood, only "phenomenal iron" can justify closed-circuit transformers.
- (4.) That the numerous tests of closed-circuit transformers, by various observers, showing high efficiency (95 per cent.) must be wrong, since they do not agree with the author's calculations.
- (5.) That the magnetising current is very large—something like 20 per cent.—even in the closed-circuit type, and for his own transformer 30 per cent.
- (6.) That with open-circuit transformers the "all-day efficiency" is much higher than with closed-circuit.

Mr. Swinburne will, I am sure, correct my preliminary statement if I do not properly interpret his views.

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\* *Journal*, xviii., p. 221.

Mr. SWINBURNE: I may mention that I have made my own Mr. Mordey, tests of iron as a rule, but I have taken Ewing, as recognised by everybody.

Mr. MORDEY: I think I shall be able to show you that all these assumptions are incorrect. Now, as to testing transformers, there are several methods. The calorimeter method as used by Professors Ayrton, Forbes, and others; certain electrometer methods; then the only other practical method that I know is that of Mr. Blakesley, who uses three dynamometers, one being a split dynamometer, in the two circuits. That is only applicable, I think, in cases where both currents can be measured. I suppose it might be used for a single circuit by having a non-inductive dynamometer to measure the volts, but I do not know whether that has been done. I must not omit to mention, also, the laborious, but no doubt excellent, curve-plotting system of testing so largely used by Professor Ryan and others in America. When I had the honour of reading a paper on alternate-current working, in 1889, I suggested a simple way of taking the losses in transformers. It hardly merits the dignified title of "method," but it seems to be fairly practicable. I have used it largely in testing transformers. Its main advantage is that no special apparatus is required. It is a calorimeter test without a calorimeter. Possibly you may remember I pointed out that if the temperature of the outside of a transformer were ascertained at full load, or at any load, and then the transformer were disconnected from the alternator and from its load, and brought or kept up to the same temperature by means of a direct current sent through its coils, the direct-current watts expended in it, and which are easily measured, would represent the loss in the transformer at the load in question. It must be noted that the thermometer reading is only required to be the same in the two cases—it shows a certain rise of temperature. It may not be the exact temperature of the outside sheath; it certainly will not be the temperature of the inside coil. But so long as the temperature attained is the steady maximum, and the transformer is of a suitable form for the experiment, there is no reason, I think, to doubt the

Mr. Mordey. correctness of the results obtained. The object is to measure the energy expended. By expending that energy in a simple way we are enabled easily to measure it. It is a simple case of substitution of a known for a doubtful quantity.

Having thus ascertained the internal loss, the load on lamps or other non-inductional resistance being measured simply as a direct-current quantity, we ascertain at once the efficiency. This is as far as I wish to go at present; but I may point out that we can get other quantities from this test. Thus the ratio of the real, or true, watts to the apparent watts gives us the cosine of the lag angle.

Having explained the method, you are now able to judge of the correctness of the results. Ten per cent. is probably a fair allowance for limit of error, and as only the loss was being measured, this scarcely affects the efficiency. For instance, if the efficiency of the transformer is 96 per cent., an error in my test of 10 per cent. only affects the efficiency to the extent of 0.4 per cent.

I may now refer to some tests. I will take one test in the first place—that of a 6-kilowatt transformer for 2,000 to 100 volts. When it was run on the high-pressure side, the secondary being open, and a voltmeter put on every now and then to see that the pressure was right, it was found that the loss in watts was 110. Then at full load the temperature rose a little, and it was found that when it had become steady the loss in watts was 205—that is to say, the same temperature was steadily maintained by 205 watts direct current. Now the 110 watts gave the loss on open circuit; the 205 watts gave the loss with full load. The load was lamps, or, rather, it was an inductionless platinoid resistance; and that this was the same as lamps was ascertained by experiment. Having carefully measured primary and secondary currents, and the resistance of the hot coils, which showed the loss in the copper to be 176 watts at full load, we have this state of things: With no load the loss is 110 watts; at full load the loss is 205 watts, of which 176 are accounted for by loss in copper, leaving a residue of 29 watts for hysteresis and eddies at full load. You will see that as the loss in copper

increased the loss in iron diminished. It is interesting to compare Mr. Mordey's this with Professor Ewing's determinations, which Mr. Kapp has tabulated, and which are also to be found in Professor Fleming's book on transformers. The magnetisation (**B**) was about 4,000. The loss from hysteresis alone, according to Ewing, ought to be 180 watts. But what are we to say to the fact that at no load we only had 110 watts instead of 180, including Foucault currents and hysteresis? I suppose Mr. Swinburne will say the results are quite wrong. [Mr. SWINBURNE: Quite right!] What are we to say to the fact that at full load, having accounted for 176 watts in the copper, the hysteresis and eddy losses are reduced to 29? I do not say that Ewing was wrong, but I do say that his results are not applicable to the conditions that exist in transformers.\*

Having experimentally determined the losses at full load and at no load, we may fairly estimate it for the intermediate loads.

*Table I.—Mordey Transformer, 6,000 Watts.*

					Efficiency.
Full load	...	...	...	...	96·7
$\frac{3}{4}$	"	...	...	...	96·2
$\frac{1}{2}$	"	...	...	...	95·1
$\frac{1}{4}$	"	...	...	...	92·1
$\frac{1}{8}$	"	...	...	...	86·7
0	"	...	...	...	1·83 per cent. loss.

\* NOTE, added later.—As Professor Ewing's determinations have been so generally accepted and applied with reference to transformers, it may be well to point out that Professor Ewing is in no way responsible for this, he never having committed himself to a statement that his slow-cycle determinations are applicable to rapidly performed cycles. He has frequently pointed out that uncertainty exists on this point. Two years ago I warned Mr. Swinburne against his own transformer calculations, and quoted a letter that I had received from Professor Ewing, in which occurred the words, "We cannot be sure that the loss by hysteresis is proportional to speed, especially when the speed is high" (*Journal*, xviii., pp. 642, 685). Professor Ewing, however, inclined to the view that the loss per cycle would be probably greater at high rates of reversal (see *Phil. Trans.*, 1885, p. 554; *Electrician*, 1890, xxv., p. 169), from effects of viscosity. It now appears that the loss is not greater, but less, at high rates than at low rates of reversal. Professor Ewing's reference to the uncertainty of experiments on quick cycles, on account of possible diminished range of magnetic change, is met in regard to transformers by finding the magnetic change from the E.M.F.—W. M. M.

Mr. Mordey.

I may mention that this transformer is not a special one in any respect, but is the Brush Company's ordinary make, commercial considerations alone preventing even better results being obtained. These results seem high, but they are no higher than other people have attained in transformers, both of my own and others' design, and I see no reason to believe that these efficiencies are not right. Now for the open-circuit loss. You see it is only 1·83 per cent. of the full load. The apparent loss at no load ( $C \times V$ ) is 3·1 per cent. Having got the true watts used or wasted, and the apparent watts put into the transformers, we are able to ascertain the lag, which comes out at between  $53^\circ$  and  $54^\circ$ .

Considering now the case of all-day efficiencies, to which Mr. Swinburne very properly calls special attention now, as he did at Birmingham, taking his basis of four hours per day at various loads, we get these values for the transformer I am considering:—

*Table II.*

Full load for 4 hours (20 hours idle).					All-day efficiency.	
$\frac{3}{4}$	"	"	"	...	...	86
$\frac{1}{2}$	"	"	"	...	...	82·7
$\frac{1}{4}$	"	"	"	...	...	68·6
$\frac{1}{8}$	"	"	"	...	...	52·6

You will see that, if half-loaded for four hours per day, instead of being 51 per cent., as the author states, it is actually 82·7 per cent., while at only one-eighth load for four hours it is more efficient than the author would allow it to be at half-load.

Almost my first tests on transformers revealed this property of diminution of the iron losses as the load rose. I found that two transformers side by side, one working at full load and the other with no load, rose to almost the same temperature. By putting the hand on them it was scarcely possible to tell which was the warmer. A careful test made at that time showed only a few degrees difference (curves shown). I found that the heat was almost constant and independent of the load. Since then I have made improvements and lowered the loss at no load,

but it has been done by going in a direction diametrically opposite Mr. Mordey. to that advocated by the author.

I am glad to be able to mention a very opportune confirmation of the results that I have described to you. In the Council Room, a few minutes before the meeting commenced, Professor Ayrton was good enough to show me a set of tests made on a transformer of my design, lent to the Central Institution by the Brush Company, for experimental purposes. That transformer is, I think, about two years old. Professor Ayrton is, I understand, going to read a paper on his tests to-morrow to the Physical Society. The tests were made by Blakesley's method. From a large number of concordant results Professor Ayrton has selected the following as fairly representative:—

*Table III.*

~.			Output (Watts).		Iron Losses (Watts).		Efficiency per Cent.
210	...	...	2,363	...	3	...	94.1
"	...	...	907	...	69	...	90.4
100	...	...	2,638	...	21	...	93
"	...	...	2,425	...	18	...	93.8
"	...	...	849	...	69	...	90

You will see that the small loss in the iron at full load is quite in accordance with my results. Such results are not new, but Mr. Swinburne rejects them because they do not agree with his reading of Ewing. But there is nothing incredible in the matter. There is no necessary physical connection between magnetism and hysteresis. Ewing himself says, for example, that the loss in iron "almost vanishes" when the iron is in a state of vibration.\*

Mr. Swinburne says if you have no heat you have no hysteresis, you have no magnetism, and no volts; but you have the volts without the heat, and there is the voltmeter to show it. And not only is the loss in the iron much smaller than Mr. Swinburne says it ought to be, but—and this is very important—the open-circuit current is only a fraction of the value he gives

\* *Phil. Trans.*, part ii., 1885, p. 554.

Mr. Mordey. it. The plant efficiency with closed-circuit is very much higher than with the open-circuit transformers. Mr. Swinburne acknowledges that in a station with 10 dynamos he must keep three of them running to feed these voracious animals of his with magnetising current.

The plant efficiency of a large station supplying open-circuit transformers must be very low indeed, as large engines must be run on light load. Mr. Swinburne acknowledges that that is a serious matter. It is a very serious matter indeed, and if he can get over it by giving us condensers it would improve his case, but we must remember that it would also improve the case for closed-circuit transformers. It would enable us to avail ourselves of that difference between real and apparent loss—between 1·8 and 3·1 per cent. But Mr. Swinburne has to make up leeway from 30 per cent. to 3 per cent. to get on even terms with us, and this he will do most easily and economically by making closed-circuit transformers. My opinion is that we should try to make our apparatus good, and then we can dispense with accessories and compensating devices, which war against the pocket.

It is a little singular that in the face of the numerous tests made by other people, and all showing similar results, Mr. Swinburne should have shut his eyes to the facts. It never seems to have occurred to him that all the other people might possibly be right, and he wrong. He has the good old English trait of never knowing when he is beaten. Even the tests by Ferraris, which, after allowing for copper losses, left less than nothing for the iron, should have been enough. Mr. Swinburne, referring to those tests, jokes about the “freezing mixture,” failing to see that when the iron loss was small a very small experimental error would suffice to place the result at the wrong side of the zero line.

I think Mr. Swinburne, on the assumptions he made, was perfectly justified in arriving at his conclusions; but if the results I have placed before you are even generally correct, I submit that they disprove and upset all those assumptions. Returning to them in order, we find—

- (1.) That the loss from hysteresis is not as heavy as the Mr. Mordey. author stated, and is not constant for all loads.
- (2.) That the losses are not ascertainable by calculation only; that even at no load Ewing's and Hopkinson's determinations are too high, and at full load those determinations are of no assistance whatever.
- (3.) That instead of being fully understood by the author, the action of the iron was not understood in the least. "Phenomenal iron" is iron whose phenomena he does not understand.
- (4.) That instead of rejecting the numerous tests by various observers, using different methods, their general agreement, showing high efficiency of transformers, amounts to a proof of their correctness.
- (5.) That the magnetising current (for closed-circuit transformers) given in the paper is several times too large.
- (6.) That facts do not support the views expressed as to "all-day efficiency."

One word in conclusion. I think Mr. Swinburne should read Goldsmith's "Animated Nature," where he would find that the hedgehog has not all its bristles at the two ends—in fact, not any there—but over all the other parts. I suggest that if he puts the bristles over all the other parts he will make a better transformer and a better hedgehog.

MR. CROMPTON: I think I can explain some of the anomalous results which Mr. Mordey has obtained when testing his transformers, by pointing out that his method of test is, I am afraid, a fallacious one. I regret that this should be so, as it possesses the merit of great simplicity; but I do not think that the temperature obtained in the rough way Mr. Mordey proposes will be any real indication of the number of watts lost in the iron. We have recently been experimenting at Chelmsford, attempting to make tests in the manner that Mr. Mordey recommends, but we have found extreme difficulty in getting at the final temperature of the interior of the iron. In the transformers we have been testing, and which are considered to be a very fair efficient type, the temperature continues to rise for very long periods up to 24

Mr.  
Crompton

Mr.  
Crompton.

or even 30 hours, so that for experiments to have any value it is necessary to employ two or three shifts of observers. I think, therefore, that Mr. Mordey has never really noted the final temperatures to which the iron of his transformers would reach if sufficient time were given. I am glad to say, however, that the tests we have made of "Hedgehog" transformers similar to those now before you have been very satisfactory as regards temperature. That of the "Hedgehog" was 120 degrees, against upwards of 200 degrees with the close-circuit transformers.

Turning now to the "all-day efficiency" of transformers, I must again strongly call your attention to the utter fallaciousness of the figures that have been put before you by various speakers. Mr. Esson and Mr. Mordey have both talked of the efficiency at half-loads as if the bulk of the work was done when the transformers are half-loaded. Now, although such a condition of lighting may be possible in shop, office, and hotel lighting, it is not possible in the lighting of private houses, which, after all, is the most important lighting of all, and to which we ought, therefore, to pay the closest attention. I have stated before, and I again repeat, that the all-day efficiency of the best modern transformers, when used for private lighting in London, does not exceed 50 per cent. I can make this clear to you by pointing out that if an average householder pays £20 a year for his electric light, it means that he uses about 70 watts an hour for the whole 8,760 hours of the year; whereas Mr. Mordey has just shown us that the transformer approximately of the right size to use for his house will require about 70 watts to excite it—in other words, that whereas the householder's consumption of electrical energy will be about 613 units per annum, the energy lost in his transformer will amount to the same figure. Such a householder probably has from 50 to 60 10-candle-power lamps installed, and requires a transformer capable of working the greater portion of them at one time, if only two or three times in the year. Throughout the whole of the rest of the year the transformer is only worked at a quarter its output for the hours of maximum demand, and during the rest of the 24 hours perhaps only one or two lights are on. If Mr. Mordey and others who

think with him wish to disprove my remarks, why do they not publish figures showing the real efficiency of the now numerous central stations working on the alternator transformer system? A few statements of fact, showing the number of watts generated by the dynamos, and the number of watts paid for by the consumers, would do more to dispel doubts which we all feel on this matter than any amount of efficiency experiments at various rates of load. At any rate, if Mr. Swinburne can in practice carry out with his "Hedgehog" transformers the very excellent results at very light loads that he has shown on the diagram before you, he will have advanced the alternating transformer system question not a little.

Several speakers have talked about banking transformers. This subject was brought forward at the time of my paper three years ago, and I still think it is the only way in which alternating transformer distribution can be efficiently carried out. I must point out that once the advocates of the alternating transformer system admit that a low-pressure network fed by various transformer stations is necessary, as they must do, then it will be very easy to compare the cost of the two systems of distribution, as that difference will mainly lie in the difference of the cost of the feeders, which in the high-pressure case consist of a high-pressure main with a bank of transformers at the end of it, and in the low-pressure case of a plain copper main without any apparatus at the end of it. It is easy to see that there is a certain distance up to which the simple main, even at the low pressure of 200 volts, will be cheaper in first cost and upkeep than the highly insulated high-pressure main with its expensive bank of transformers at the end of it. Mr. Swinburne has stated that accumulators are expensive to maintain, and from his tone I judge that he feels confident that they compare very unfavourably in this respect with transformers. Here my experience is against him. People talk about transformers as if they were non-moving machinery. This is not the case. We are gradually beginning to appreciate the fact that the parts of the ironwork of a transformer have such a tendency to move among themselves at an extremely rapid rate, that the wear and

Mr.  
Crompton.

Mr.  
Crompton.

tear, both to the iron and the insulation, from this cause is very considerable. At any rate, no one would put the upkeep of transformers as they now are manufactured at a figure lower than that of accumulators. Mr. Swinburne says that no one knows anything about the cost of upkeep of accumulators. If he had applied to me, I could have given him accurate figures. I can tell him, further, that the reason why accumulators have not been popular in America, has been chiefly because they have not as yet succeeded in making a good accumulator in America; and that their use is not nearly so valuable in old well-established low-pressure stations which have a constant day and night load, such as is the case with many of the earlier Edison American stations. At any rate, it is easily capable of proof that a certain percentage of storage plant added to a low-pressure station worked under the conditions of load which prevail in English towns, greatly reduces the cost of production of the electrical energy. I have been working at this subject for some time past, and I hope to be able soon to make my figures public.

Mr. Adden-  
brooke.

MR. G. L. ADDENBROOKE: I think that Mr. Swinburne is under a wrong impression when he states that probably no existing company intend changing their system of supply, for I remember that when Mr. Ferranti took out his first patents for transformers, the subject of sub-stations and low-pressure distribution was thoroughly appreciated by him, and was more than once talked over between us. At that time, however, it was impossible to found sub-stations; still, it was always the intention of Mr. Ferranti, as well as of the House-to-House Company, to adopt this system ultimately whenever they could. The cutting out or in of transformers at such stations can be effected either automatically, or by hand on the lamp-lighter principle, and with either method a difference of 1 or 2 per cent. in the efficiency of the transformer is a matter of little importance.

Mr.  
Evershed.

MR. S. EVERSLED: I will do what Mr. Addenbrooke suggests, and bring the discussion back to the paper. One of the great merits of Mr. Swinburne's papers seems to me to be that they lead to good discussions, and Mr. Mordey is never more effective than when he is crushing Mr. Swinburne. The first thing I notice is

the admirable description Mr. Swinburne gives of the mode in which alternators act as motors or generators, and by reference to the diagram I think it is possible in a very short time to see exactly Mr. Swinburne's meaning, and to see that his explanation of alternators running in parallel is the true one. It is far easier to understand the action of alternators from the armature reaction theory than from what Mr. Swinburne calls the orthodox theory. Mr. Swinburne makes one rather remarkable proposal, namely, that alternators either running as generators or motors might be compounded by passing the main current redressed round the field magnets. I daresay he has some happy way of overcoming sparking at the commutator, but I should like to hear from him how he proposes to do it.

Mr. Swinburne mentions that according to his own tests the hysteresis loss in commercial iron is often very much larger than the figures given by Professor Ewing. I also have made many tests in conjunction with Mr. Vignoles, and we hope to publish them some day; but in the meantime I may mention that we have never found a sample of wrought iron in which hysteresis waste was more than 25 per cent. greater than Ewing's figures for the best iron. Charcoal iron, or mild steel, commonly used for making cores for transformers, is, roughly, 5 per cent. worse than the figures given by Ewing. In fact, if when designing a dynamo or transformer we reckon the hysteresis loss at 20 per cent. in excess of Professor Ewing's values, we shall be well within the mark.

Mr. Esson has already noticed the peculiar idea of closed-circuit transformers which Mr. Swinburne has. He gives the full particulars of a "Hedgehog" for a certain output, and then compares it with a closed-circuit transformer. I do not know whether it is a real transformer or not.

MR. SWINBURNE: I simply took that as an average transformer in the market, and I think anyone looking at it will allow it is fair.

MR. EVERSLED: I do not allow it is fair. You must allow the same drop in volts at full load for both transformers, and the same

Mr.  
Evershed.

Mr.  
Evershed:

insulation ratio, and so on ; I do not know whether Mr. Swinburne did that.

Mr. SWINBURNE: If you want to find that argument followed out, you will find it in my British Association paper, where I took the best forms of closed-circuit transformers.

Mr. EVERSLED: I have read that paper, and Mr. Swinburne certainly gives better results there for closed-circuit transformers than he does here. He should not compare a transformer designed by anybody with a transformer designed by himself. Each should be the best of its kind. A 50-light closed transformer with only 400 turns on the primary and a loss of 120 watts in the core is absurd. Suppose it had 2,000 primary turns, the area of the core would be reduced to about one-fifth, keeping the induction density the same. I reckon, very roughly, you may get a loss of about 40 watts in the core instead of the 120 given by Mr. Swinburne ; and the exciting current, instead of being .2, would be more like .05 ampere. But, after all, suppose the "Hedgehog" does take a large exciting current, if Mr. Swinburne can produce a really commercial condenser, there is no reason why open-circuit transformers should not be used. In fact, if you take any closed-circuit transformer, no matter of what type, and convert it into an open circuit without the "Hedgehog" arrangement, the losses in the core must be very much diminished. It becomes a question, then, whether condensers can be made a commercial success. But suppose in a few years' time condensers are made of large capacity, perfectly easy to manage, cheap, and practically everlasting at high tensions, why not go the whole hog? Throw away the whole core ; do without it altogether. When there are no core eddy-currents and no hysteresis losses, you can raise the frequency ; and so far as the transformer itself is concerned, there is no reason why it should not be raised to 200 or 300 periods per second. At such a high frequency it is easy to make a practicable coreless transformer in which the constant losses would be very small indeed.

The discussion was adjourned until March 12th.

A ballot then took place, at which the following candidates were elected :—

*Member :*

William Tregarthen Douglass, M. Inst. M.E.

*Associates :*

George B. Chutter.

Frederick Mowbray Donne.

James King.

Gerald W. Partridge.

W. Chetham Strode.

Victor Wandrus.

Francis P. Walker.

*Students :*

Herbert James Allen.

Horace Louis Petit Boot.

Charles Arthur Gawthorp.

Henry Geoffrey Haig.

Thomas Archer Rose.

The meeting then adjourned.

The Two Hundred and Nineteenth Ordinary General Meeting of the Institution was held at the Institution of Civil Engineers, 25, Great George Street, Westminster, on Thursday evening, March 12th, 1891—Professor AYRTON, F.R.S., Vice-President, in the Chair.

The minutes of the Ordinary General Meeting held on February 26th, 1891, were read and confirmed.

The names of new candidates for election into the Institution were announced and ordered to be suspended.

The following transfer was announced as having been approved by the Council:—

From the class of Associates to that of Members—

The Rev. F. J. Smith, M.A.

Donations to the Library were announced as having been received from Sir David Salomons, Vice-President, to whom the thanks of the meeting were duly accorded.

The CHAIRMAN: We will now resume the discussion on Mr. Swinburne's paper on "Transformer Distribution."

Mr. Trotter. Mr. A. P. TROTTER: Mr. Swinburne has been criticised by more than one speaker already for dwelling on figures instead of facts; and this criticism is perhaps hardly fair, because in dealing with the armature reactions in alternating-current dynamos, it is better to be too early than too late. Mr. Swinburne arrived at very important results with regard to armature reactions of constant-current machines some time before they were published in his paper before this Institution; but they were not published in time to prevent some very serious mistakes in the building of large Gramme dynamos. Perhaps the most important part of the paper is that which relates to the tests of transformers, and we have some four different methods before us. Some of the earliest tests of transformers, I believe, were made with calorimeters, and my recollection of laboratory work is that there is no laboratory measurement

which requires so many and such difficult corrections as **Mr. Trotter** calorimeter work. To measure an ordinary glow lamp you want a calorimeter at least the size of a hat-box, and to measure a dynamo you want one of the size of a cab. You must determine the temperature of the outside of the calorimeter; and to do this you must be sure that the heat is uniform all over—that is to say, the isothermal surfaces must be parallel to the surface of the casing surrounding the transformer. Then there is the wattmeter test, which is almost too new to criticise; probably there is a great deal in it, but it is too early to judge. Then the split dynamometer method has received considerable attention, and has been brought before us with some very complex mathematics. I can point to one difficulty, and that is as regards the stray field leakage of the transformer. Now some of us heard a most interesting lecture at the Royal Institution last week by Dr. Fleming on alternating-current effects, and some of the most striking experiments were due to the peculiarities of the stray field. The leakage of lines of force took place in quite a different manner from that to which we are accustomed with constant-current magnets. I just throw out the following suggestion—it may not be worth anything. In some of Professor Elihu Thomson's experiments, repeated by Dr. Fleming, copper behaves in many respects in an alternating magnetic field in very much the same way as iron does in a continuous field. A copper disc sets itself edgewise to the lines of force. It may be worth considering, therefore, whether we could not explore an alternating-current field by means of copper filings instead of iron filings, or some such method. At present we know very little about the stray field line for the transformer or for alternating-current machines.

Mr. Mordey tried the thermometer method many months ago. I will allude to only one defect that is very apparent. Mr. Mordey first heats his iron by hysteresis and Foucault currents, and then heats his copper, and using his thermometer as a mere indicator, by a zero method, he assumes that the conditions in the two cases are identical, and that the distribution of temperature is perfectly uniform all over the transformer. If you heat

Mr. Trotter. iron first and copper afterwards by direct current, the result may tell, perhaps, somewhat against what he wishes to prove; but at the same time it is very essential that the distribution of temperature should be perfectly uniform all over the transformer, and that must be achieved by putting it in some sort of case, and that in another case, and so on, until the distribution is uniform. It is impossible to estimate how the heat will flow. The isothermal curves, at any rate, must be parallel with the outer casing. Only then, I think, can you take the thermometer and say that the conditions for two successive experiments are the same.

The loss in iron, I understand, Mr. Mordey arrived at ten months ago, and his results are confirmed by Professor Ayrtton's recently published experiments; and having been arrived at by two methods, they are all the more interesting. But, speaking for myself, I cannot understand either of them properly, and cannot help owing to some hesitation in taking these rather extraordinary results. I want to have some reasons or explanations of these results. I have always understood that the induction is practically constant in a transformer, save for the small drop that takes place. I want to know if that is still so under that new hypothesis. Of course, we know the secondary current demagnetises slightly, but then the primary current increases, and there should be some little further information as to what probably goes on in the transformer, to fit this new fact, viz., that the loss drops down to 3 watts on full load.

Since we met here last, the knowledge of alternating-current work has been very materially increased at two meetings of the Physical Society, where two papers, differing largely in their character, have been submitted—one from the mathematical, and one from the practical point of view. We want to know, besides this, if the new hypothesis is to fit with Ewing's theory of magnetism, which anyone can understand. There has been one thing neglected, apparently—maybe I only have neglected it—that is, the difference of hysteresis at high frequencies and at low frequencies. Ewing's curves are the result of very leisurely, if not tedious, experiments, and we are not justified in assuming that

when we come to high frequencies the hysteresis will be the same. **Mr. Trotter.** I do not think that in the least affects the decrease of loss in the iron on heavy loads. It seems very probable that hysteresis depends very largely upon the frequency. I have in my pocket a piece of pitch, which is quite soft and plastic. If I take a tuning-fork and strike it on the table, those who are near will hear the ordinary note. If I put it on the piece of pitch, it is exactly as loud. As this piece of pitch is very easily bent and moulded, the internal friction, or hysteresis, in that piece of pitch is very considerable. It transmits those vibrations at the rate of 426 per second perfectly easily, and is, therefore, practically, perfectly elastic. But at the same time it is like so much putty in my hands. It may be possible that in iron, dealt with slowly by Ewing, there is hysteresis, but at high frequencies the internal friction decreases.

**Mr. G. C. FRICKER:** I shall not attempt to discuss the many **Mr. Fricker.** points of interest in the first part of Mr. Swinburne's paper, but I should like to make some remarks on that part of it which deals with the comparison of open-circuit and closed-circuit transformers.

It is evident that in estimating the iron losses in the small transformer in Fig. 11 at 120 watts, Mr. Swinburne is in full accord with those writers and teachers on the subject who hold that the iron loss in transformer cores is a constant for all loads.

There is no doubt that in taking this view the author of the paper is in exceedingly good company; indeed, I believe that nearly every recognised authority on the subject shares it with him. This assumption, however, was challenged at the last meeting by Mr. Mordey. I confess that I do not agree with the majority, and I should like to try and point out what seems to me to be a great confusion in the accepted explanation of the actions which occur in transformers.

I will first draw attention to a particular geometrical construction which was offered by Mr. Kapp as an explanation of the actions taking place in a closed-circuit transformer in which the magnetic leakage was negligible, and in which the self and mutual induction were, therefore, synonymous terms. Fig. 1 is a

Mr. Fricker. diagram illustrating Mr. Kapp's views. In this, I think I may say famous, diagram Mr. Kapp deliberately begs the whole

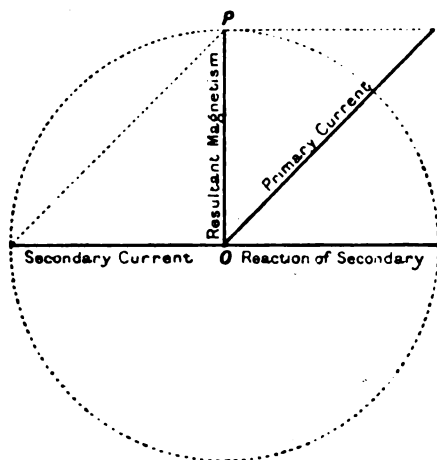


FIG. 1.

question. He draws a circle whose radius is supposed to represent the maximum value of the resultant induction, and then proceeds to build up his component forces upon it. In so doing he makes what seems to me to be a most remarkable and sweeping assertion—viz., that the total, or resultant, induction,  $OP$ , is in quadrature with the secondary current, because,

as he says, the secondary current is in consonance with the impressed secondary E.M.F. This construction has been acknowledged by Dr. Fleming,\* who, in his book on alternate-current transformers, quotes and enlarges upon the whole of Mr. Kapp's work on this subject. The book has been ably reviewed by Professor Thompson, so I suppose he also agrees with these statements.

I will now refer to Mr. Blakesley's diagram on the subject, which, by the way, is also endorsed by Dr. Fleming. Mr. Blakesley does not proceed from the same starting point as Mr. Kapp, and his results appear to me altogether different. Fig. 2 is a modification of Mr. Blakesley's diagram with the forces all set round the centre,  $O$ , so as to make their phase differences the more apparent.

Starting with the impressed primary force previous to any reaction from the secondary, the primary current is in quadrature with the force of primary self-induction, and is, therefore, plotted within the semicircle, as shown. Upon the force of primary

\* Fleming, page 275.

self-induction, which in closed-circuit transformers is also the mutual induction, is constructed the secondary triangle, in which

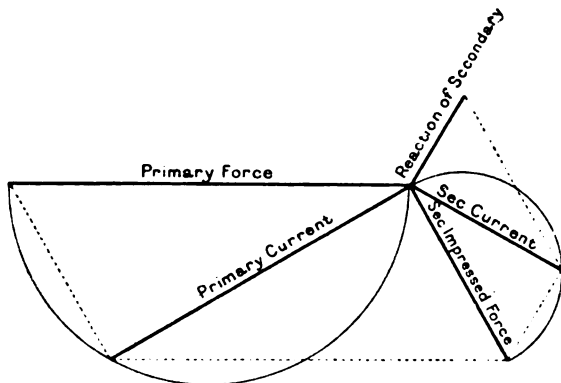


FIG. 2.

this force of mutual induction is the impressed force. This gives us the secondary current and secondary force of self-induction, or the reacting force on the primary, from which, finally, the true impressed force on the primary is found.

The reasoning applied in the construction of this diagram appears to be sound enough, and, so far as I know, has not been contradicted since Mr. Blakesley first published it in 1885. But, by assuming the necessary adjustments of the primary and secondary currents, Mr. Kapp's construction may also be made to appear as one of the most charming geometrical exercises which one could wish to see. Everything fits in, and therefore everything must be right; but everything has been taken for granted to start with. We are, however, in this dilemma: Mr. Kapp says the secondary impressed force is in consonance with the secondary current;\* Mr. Blakesley says the secondary impressed force is in quadrature with the primary current.† Therefore, if they are both right, the phase difference between the primary and secondary currents is always 90 degrees. I think that at least one of these statements must be wrong, and I ask Mr. Swinburne whether he accepts either of them.

\* *Journal*, vol. xvii., p. 101.

† See "Alternating Currents of Electricity," 1st edition, Construction, on page 19.

Mr. Fricker.

At the last meeting Mr. Mordey brought forward some tests which point in the most forcible manner to the real facts in connection with the magnetisation losses in transformers—tests, too, which appear to have been completely verified by Professor Ayrton quite independently. Now I think, in view of the evident contradictions of recognised authorities on the theory of transformers, that we ought to pay the greatest possible respect to experimental demonstrations, and I would advise Mr. Swinburne not to set such tests aside too lightly.

Mr. Crompton does not disbelieve in the method of Mr. Mordey's tests, but finds that after 30 hours the temperature of those transformers on which he experimented was still rising. I do not wish to say anything disrespectful about those particular transformers, but the melting point of some samples of iron, especially "phenomenal iron," is very high. For my part, I think that, like all his work, these latest experiments of Mr. Mordey's are beautifully simple, effective, and thoroughly practical. I was not altogether unprepared for the result. Some considerable time ago Mr. Mordey told me of the peculiar fact of the iron losses in his transformers on full load being very much less than was observed on open circuit.

With regard to the practical outcome of all this, so far as the comparison of closed, and open-circuit transformers is concerned, of course Mr. Swinburne may take refuge in the argument that what is sauce for the goose is sauce for the gander, and that therefore his transformer will be equally benefitted if Mr. Mordey is right; but although it may be sauce for the gander, I do not think it is sauce for the "Hedgehog," as the large quantity of current consumed by this creature between meals is very inconvenient, and makes it necessary to keep a very big larder; moreover, the plant efficiency of the system is materially lower even during the hours of full load.

It is difficult to conceive the mechanism which must exist within the transformer if Mr. Mordey's figures are true; but in that case it is certain that one of the most interesting and important points in the recent development of our science will have been opened out, because it will have been shown that it

is not necessary under all circumstances for the generation of an alternating electro-motive force in a circuit to be accompanied by the fulfilment of the conditions hitherto usually associated with our ideas of magnetic change in iron. Mr. Fricker.

It must be true that a cycle of magnetic force exists through the secondary, but it may be that the reacting and reciprocating forces transmitted when both circuits are operative modify and partly neutralise the effects of hysteresis.

There is just one other point I should like to mention. On page 5, in speaking of the effect of the armature current in alternators, Mr. Swinburne says: "I think, to call all this 'self-induction,' and to assume the harmonic law just to make the calculation easy, is disguising the real nature of the action, and may lead to very serious error." This is not the first time Mr. Swinburne has protested against the assumption of the harmonic law in dealing with these questions. I learn that quite recently Professor Ayrton has given an elaborate analytical proof of the truth of calculations based upon the assumption of this law; and I should like to state that some months ago I mentioned to Professor Thompson and several other members of this Institution some simple considerations which were capable of proof, and which had led me to the same conclusions as Professor Ayrton.

It would appear reasonable to suppose, since any function is capable of being expressed as a number of harmonic functions, according to Fourier's theorem, that in making any investigation into the effects of, or reaction on, such complex functions the component harmonics may be treated separately.

For instance, we may take a simple case in which two harmonic functions form the components of the complex function, as shown in Fig. 3. Here we may investigate each sine curve separately, and so determine the properties of the complex curve. To do this, imagine the two waves to be generated in different parts of an electric circuit by some such mechanism as is suggested by Dr. Fleming on page 84 of his book. Let the coils rotate at different velocities in fields of the same strength; let the areas of the coil be as  $1 : \frac{1}{4}$ , and the velocities as  $1 : 2$ , respectively: then

Mr. Fricker. the electro-motive forces are in the ratio  $1 : \frac{1}{2}$ , and with a given current the E.M.F.'s of self-induction will also be in this ratio.

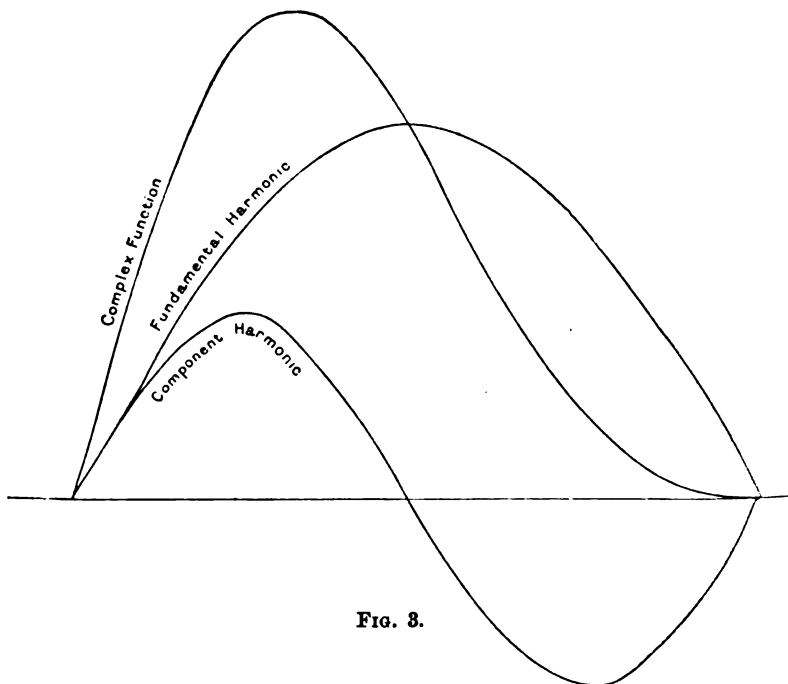


FIG. 3.

Consequently, if the maximum values of the impressed E.M.F.'s are plotted on the horizontal line in Fig. 4, and the maximum value of the E.M.F.'s of self-induction with any given current are inscribed within two semicircles drawn on these impressed E.M.F.'s, the angle of lag will be common to both functions. Thus each component harmonic wave of the actual current-wave lags behind its impressed component force by the angle  $\theta$ ; obviously, therefore, the resultant current-function must lag behind its resultant impressed force by this same angle. The angle of lag in any complex function can therefore be determined by finding that belonging to one of its component harmonics. In the measurement of alternating currents the Cardew voltmeter or Siemens dynamometer gives a reading which, if the wave were truly harmonic, would be proportionate to half the square of the maximum value of the current. From the reading we can plot the equiva-

lent harmonic wave of whatever function we may happen to be measuring, and may treat it as the fundamental wave of the set

Mr. Fricker.

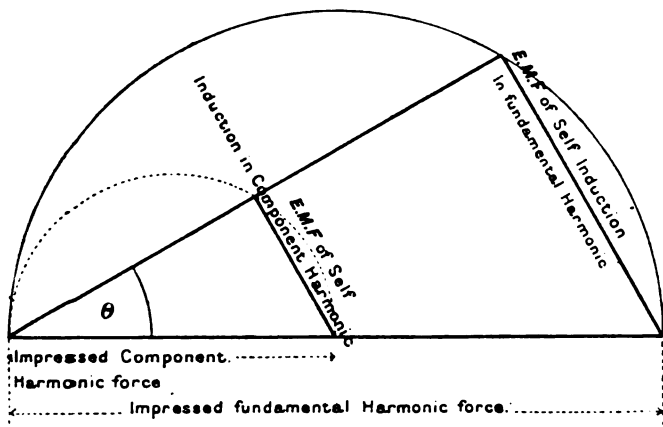


FIG. 4.

of superimposed waves of which the complex function is composed. This is justifiable, because it is obvious that by a suitable combination of harmonic waves with this fundamental wave, any shaped wave could be produced. But it has been shown that the angle of lag proper to any one harmonic component is the angle belonging to the complex function as a whole, and therefore the angle calculated from the assumption of the harmonic wave indicated by the reading of the Cardew and Siemens instruments is absolutely correct.

The same reasoning will apply to secondary waves when the coefficient of self and mutual induction coincide—that is, when there is no stray field in the transformer.

Dr. FLEMING: I had not the opportunity of hearing the discussion on Mr. Swinburne's paper last week, and in the few remarks I have to make I shall ask indulgence if I cover ground already gone over in the debate. Mr. Swinburne has certainly given us enough material for several evenings' discussion, and has thrown down the glove on a large number of questions on which many of us differ from him and differ from each other. He has developed his theory of parallel working of alternators as against what he calls the orthodox theory, but I am bound to

Dr. Fleming

Dr. Fleming say it seems to me he merely puts forward old views under a new name. "Armature reaction" is only another name for the mutual induction between the armature and fields, and as such its existence has always been recognised. In all these questions a distinction should be drawn between the instantaneous and the mean inductance of the circuits. If we take a diagram which I have elsewhere called an "indicator diagram" of a transformer—that is, a series of curves such as those drawn by Professor Ryan, showing the exact form of the curves of electro-motive force, magnetisation, and current in a transformer—it is easy to show that the instantaneous inductance at any instant is the ratio of the time rate of change of the ordinate of the magnetisation curve and the time rate of change of the ordinate of the current-curve at that same instant, and from the slopes of these two curves as drawn on an indicator diagram the inductance at any instant can be found. The diagrams show that the inductance varies in a transformer or alternator circuit from instant to instant, and is a periodic function of the time. It is the average value of this continually varying inductance which has to be considered in transformer and alternator problems. As regards practical working in parallel, everything that I have seen since the reading of Mr. Mordey's paper confirms the general accuracy of the views then advocated, with this qualification—that it is not small inductance, but a small ratio of mean inductance to resistance in the armature of the dynamo, which seems to be one condition of successful parallel working.

The Ganz alternators—so extensively used on the Continent, and especially in Italy—work perfectly in parallel, as at Leghorn, Rome, Marienbad, and elsewhere, and they are not machines of particularly small armature inductance. I cannot agree with Mr. Swinburne's contention that a pressure of 2,000 volts is much more difficult to work with than 1,000. It requires some different details, but the number of stations actually and safely working at the higher pressure shows that this increased difficulty is not felt in practice. I doubt whether the use of low-pressure alternators and step-up transformers would be more economical, for the additional waste in the

transformers will certainly be an appreciable matter, and their cost will, on the average, exceed the figures given by Mr. Swinburne. Then, coming to Mr. Swinburne's chief contention as to the superiority of the "Hedgehog" transformer, this really reduces itself to a claim that the efficiency curve for the open-circuit form is more square-shouldered than for the closed-circuit form. This is not a question to be settled by argument, but by experiment; but such experiments cannot be satisfactorily made with a wattmeter, even if the fine-wire coil is made partly non-inductive. Mr. Swinburne admits that the "Hedgehog" requires a larger exciting current, but he underrates the disadvantage this will be in practical work, entailing, as it will do, a large outgoing current from the station during daylight hours, and a much lower plant efficiency. With respect to sub-stations, it may be generally said that designs which look very captivating on paper for low-tension distribution have often a more unsatisfactory look when taken in the severe light of practical work; but in all cases, whether it is found better to use distributing transformers or small sub-stations, the high-pressure lines should be so run as to terminate and branch from points at which it will be convenient ultimately to place transformer sub-stations.

The author of the paper has, I think, omitted to deal with one matter of great practical importance, and that is the "drop" in the secondary circuit of his transformers; because transformer efficiency is not the only factor in determining the value of a transformer, and without a small drop a transformer becomes a very unpractical thing.

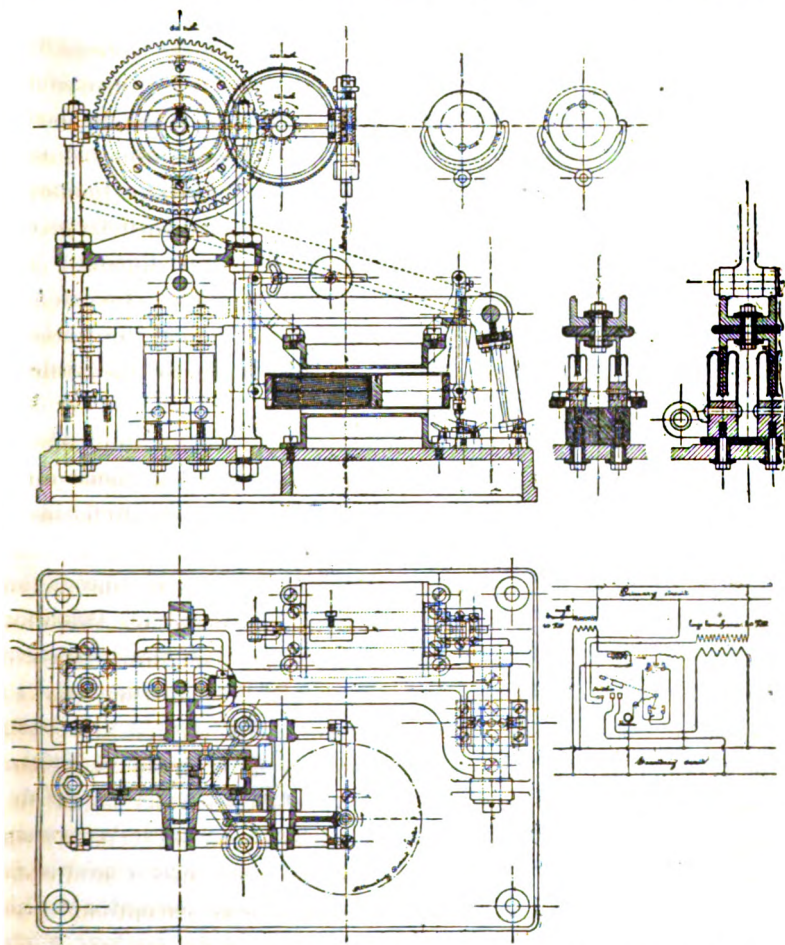
Mr. GISEBERT KAPP: The paper covers so much ground that each speaker can only discuss a very small part of it. I will only take up two points. First, the question of open *versus* closed magnetic circuit, which I shall endeavour to put in very plain, common-sense language, as opposed to a deep mathematical treatment. You see a "Hedgehog" on the table before you. Let it be excited, and measure the exciting current. Mr. Swinburne tells you that the exciting current is very nearly in quadrature with the E.M.F., and therefore wastes very little

Mr. Kapp. energy. Now give the transformer a coat of wires—make a shell, so to speak; then you have a closed-circuit transformer. I ask Mr. Swinburne to answer me this question—Will this transformer require a smaller exciting current? I think he will say, Yes. If in its original naked form the “Hedgehog” takes 3 amperes, as soon as we give it a shell it takes 2 amperes—a clear gain of 30 per cent. in the matter of plant efficiency. Now comes the other question—Will the lag be decreased by giving this outer covering of iron? I think not, because the waste field will be reduced, and the induction throughout the mass of the core and shell will be much more uniform than it was in the core alone previously. But a uniform induction means a lower induction throughout, as compared with the necessarily high induction in the middle of the “Hedgehog” core before, and this must tend to increase, rather than decrease, the lag. Therefore, by giving the “Hedgehog” a shell, we increase the plant efficiency, and at the same time decrease the waste of power.

If Mr. Swinburne in his reply can refute this argument, he must yet meet another argument before he can make out a good case for the open-circuit transformer. Virtually he says: “The closed-circuit transformers are all very well on a heavy load, but they are very inefficient on a light load; and as you are running 20 hours out of 24 with light or no load, therefore a closed magnetic circuit is the worst possible form. Here is my ‘Hedgehog;’ it has a high all-day efficiency, because there is less iron in it, and therefore less waste of energy.” But at the same time he tells us that it requires a large magnetising current. Now, during more than half the time it is the magnetising current only which goes into the transformer, and to satisfy the ravenous appetite of the “Hedgehogs,” all over the district served, and all day long, we must send a large current out from the station. Whether this current represents energy or not, the fact remains that to produce it we must keep large engines running. I have been rather dissatisfied to hear from Dr. Fleming that he thinks sub-stations are impracticable on account of capital outlay. I some years ago had the honour to bring

a paper before this Institution in which I advocated the system of sub-stations, not as a new idea, for other engineers had thought of them before me; but since then the idea of sub-stations has taken rather a stronger hold of some electrical engineers; and although I fully admit that as long as the operations of an electric light company are tentative it would be

Mr. Kapp



AUTOMATIC SWITCH FOR TRANSFORMERS, FOR 50-K.W. SUB-STATION OR  
1,000 8-C.P. LAMPS.—Full Size.

folly to go and build large sub-stations and put down low-pressure distributing mains for alternating currents, it is the  
VOL. XX.

Mr. Kapp.

system which I am firmly convinced must be adopted eventually. We must work on lines comparable to direct-current distribution. In this system we have small stations serving districts of a quarter or half a mile radius, and the service is satisfactory and economical. Now let us assume that instead of carting coal to these small stations we supply them with electric energy from a larger station: we can then distribute in the same manner. It is true that we shall have the loss by transformation, but that can be made very small—I might say exceedingly small in comparison with the present system, when you give a transformer to every consumer—for the simple reason that you can at a sub-station have small and large transformers. It would not pay to put into your consumer's house one transformer for five lamps and one for 25 lamps. An automatic apparatus on so small a scale would very likely go out of order. But at the sub-station you could do it; and, in fact, if you want sub-stations it is essential to have automatic apparatus. Perhaps there would be three or four transformers in the sub-station—one for 500 lamps, one for 1,000, and one for 2,000, and so on. These must be joined into the circuits and withdrawn automatically as corresponds to the demand for current. The diagram shows an apparatus which I have designed for this purpose.

Mr. King.

MR. KING: I feel somewhat disappointed that the discussion upon this paper—the title of which is “Transformer Distribution”—has degenerated into a discussion of the maximum efficiency of transformers. I take it that the majority of engineers have heard sufficient about the efficiency of the transformer *per se*, and want to know what is going to be the efficiency of the alternating transformer *system*. I think Mr. Swinburne stated the absolute truth when he says the system of putting transformers in the houses of consumers cannot in the long run be a paying one. But I take exception to his estimate of the daily load. He says the load will be four hours at half-load per day. It is, however, a fact that in summer time in some districts in London the consumption will not be one hour of half-load per day; and if Mr. Swinburne takes the fairer average of three hours' half-load per day throughout the

year, then I think the efficiency of the transformer distribution Mr. King. will be something less than the lowest figure mentioned during this discussion. I note with considerable satisfaction that Mr. Swinburne mentions the utility of sub-stations. So long ago as 1883, in a paper I read at Colchester, I started with the statement that I believed the principle of centralisation—*i.e.*, fixing all the generating and distributing machinery in one place—was the one stumbling-block in the way of electrical distribution on a commercial scale. As time has gone along it has been proved that, so far as those responsible for that scheme were concerned, we had grasped the difficulty of distribution, by high or low tension, so far as economy was concerned. The remarks that I can have to make on a paper of this description are, of course, very small; but Mr. Swinburne has endeavoured to strike his customary blow at secondary batteries, and, in consequence, I feel bound to answer. I shall be very glad to hear from him, or from any representative of alternating distribution, (1) how many stations there are in England which are now working without the use of storage; and (2) how many times during last year those stations have had to shut down for some cause or other—how many times, in fact, the consumer has been put to the inconvenience of having his light cut off. Storage batteries are now a commercial article, whatever they were a year or so back. When the representatives of alternating systems have learnt how to apply storage to their arrangements—to modify their arrangements so as to facilitate its use—we shall find them becoming the most staunch supporters of what, in the long run, will prove an absolutely necessary adjunct to every electric supply station.

Mr. JOYCE: I would like to ask one question with regard to Mr. Joyce. the experiments of Mr. Mordey, and those of Professor Ayrton cited by Mr. Mordey. Mr. Mordey and Professor Ayrton both made some experiments with transformers, from which it appears that with an increase in the output the expenditure in the iron decreases, and we get a curve something like this [*indicating*]. We have at very large output a very small waste in the iron. That is to say, starting at no output, we have

Mr. Joyce. a large waste in the iron, and as the output increases, the waste in the iron diminishes. I wish to ask Mr. Mordey and Professor Ayrton whether the slope of this curve is such as to give any reason to suppose that it cuts the zero line and shows "negative waste," or does it tend to become horizontal?

Mr. MORDEY: Yes.

Professor AYRTON: It goes up again.

Major CARDEW: On the subject of sub-stations, I think different people have different ideas as to what is meant by "sub-station;" because a sub-station on Mr. Kapp's system is certainly an expensive affair, and the exact situation of it is difficult to determine at first. But I do not think it follows that it is necessary to put a transformer on every ordinary consumer's premises—perhaps on a shelf where any enterprising individual might investigate the primary circuit. There is no reason why a small lean-to should not be erected adjacent to the consumer—get the permission of the consumer—and then you may put down not only one transformer, but enough, probably, to supply several people around, and in that way gradually get up a sub-station.

Mr. Mordey. Mr. MORDEY: I crave indulgence, having already spoken in this discussion. Referring to Mr. Trotter's criticisms on my plan of testing transformers, and on what I said about losses in iron, may I point out that the magnetic leakage supposition does not help him at all, as he will acknowledge when he reflects that sufficient leakage to account for the marked reduction of hysteresis loss at full load must be accompanied by a lowering of the secondary volts far below what we know actually takes place.

As to the thermometer test, I am afraid I have failed to clearly explain the principle of the test. It does not seem to me to matter whether the heat is generated in the iron surrounding the copper, or in the copper surrounded by the iron, or in both. For the same steady loss, however produced, the same steady surface temperature must be ultimately attained. It should be remembered that in my transformer the loss is very evenly distributed, making the method easily applicable; but it is only fair that I should say the thermometers were placed in the tests

in question just over the middle of the sheath—the place that *Mr. Morley* gets slightly hotter with a given alternating-current loss than with the same direct-current loss. This would slightly tell against the transformer, causing the efficiency to come out a little lower than the true efficiency. I am on this account quite sure that my figures for efficiency are, if anything, a slight understatement. By using two or three thermometers arranged to give a mean for the whole surface, very accurate results may be obtained. The question as to whether this method—or any method—will give correct results depends very much upon who makes the experiments. Of course one has to use one's common-sense in these matters; for instance, the thermometers should have spirit, and not mercury, in them. Indeed, a very good magnetic leakage detector consists of a spirit thermometer and a mercury thermometer side by side. When there is leakage, eddies are set up in the mercury, which is thereby heated, and therefore reads higher than the spirit; the difference between the two thermometers affording some rough index to the amount of leakage, or the strength of the stray field.

This mode of testing takes a long time, but so do all calorimetric tests. I do not put the plan forward as a very convenient one from a laboratory point of view, but think it has this great advantage—one does actually get the practical working conditions of the transformer. On the one hand, it avoids the boxing up and artificial surroundings of a calorimeter, which may give a distribution of heat quite different from practice. And, on the other hand, it prevents the errors that may arise from tests taken quickly by ordinary readings of instruments, where time is not allowed for the transformer to attain its ultimate working state. It is only by long-continued practical tests, such as running continuously on circuit, that the true character of the apparatus can be ascertained. In central stations this is not difficult, as the machinery is always running. You will see from the curves —[*curves shown*],—that one of my 2-H.P. transformers got to its maximum temperature in six to seven hours.\* Larger sizes

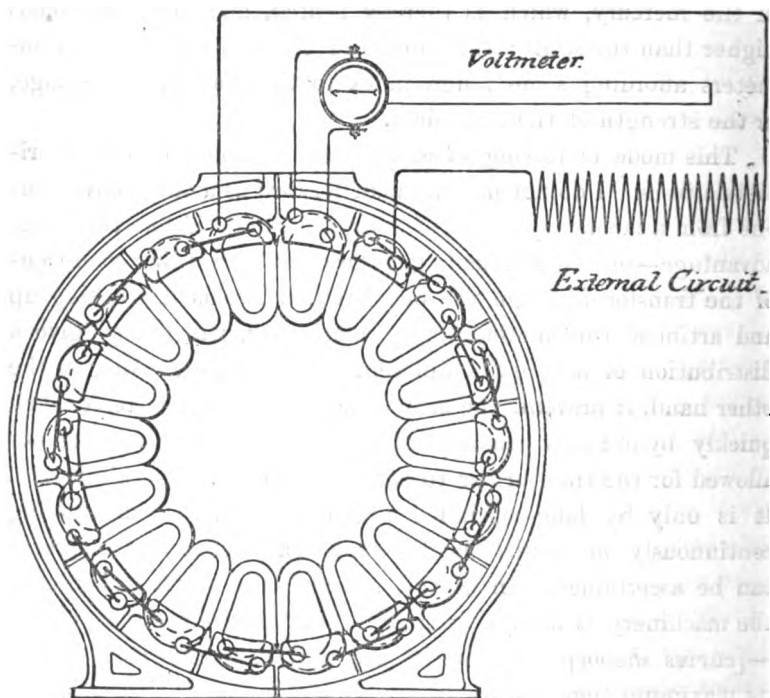
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\* *Journal*, xviii., p. 607.

Mr. Morley: take longer, but the time may vary very much with the design. Indeed, I have seen transformers that seem to have been deliberately designed on the supposition that they were never to be run more than a few hours, for if that time were exceeded on full load they were not safe.

To the remark that experiments made to prove a theory are unreliable, I need only reply that, in the first place, I have not advanced a theory, but merely stated a fact; and, in the second place, that it was the agreement between very numerous tests that revealed the fact, and forced me to believe in it.

Mr. Trotter's experiment showing that a piece of pitch transmits quick vibrations is interesting, but, I venture to think, without bearing on the question at issue, since the hysteresis loss is present at no load, but absent, or lessened, at full load, showing that the high periodicity is not responsible for the effect.



One other point, which time did not allow me to mention at the last meeting: I refer to an experiment bearing on the question

of armature reactions that I made nearly two years ago, and which Mr. Mordey. may now be of interest. It arose out of the paper I had the honour of reading here. The object was to find what effect the armature current had upon the field magnet of one of my alternators. One of the coils of the armature was disconnected from the rest of the armature circuit, and connected to a Cardew voltmeter. The other coils were led to the external circuit. The accompanying figure shows the arrangement. The field was steadily and normally excited, and, the alternator running at its proper speed throughout, the current in the external circuit was increased to the full load—that is, from 0 to 20 amperes, the normal full load—the pressure being 2,000 volts. The voltmeter on the single coil gave the same reading throughout. This appears to show that in this case, at any rate, the reaction of the armature on the field was inappreciable.

Mr  
Evershed.  
Mr. S. EVERSLED: May I explain that in Professor Ferraris's experiments the drop in efficiency was due entirely to the fact that he was working with transformers having an *open* magnetic circuit, and on a large load the  $C^2 R$  loss begins mounting up at a great rate, finally becoming far larger than the losses in the iron core, and under those conditions the efficiency naturally falls below its maximum.

The CHAIRMAN: Before calling on Mr. Swinburne to reply, I Professor  
Ayrton. will make one or two remarks myself. Not having been able to be present at the last meeting, and not having yet seen any abstract of the discussion that occurred on that occasion, I shall confine myself to one or two points only, in order to avoid the possible repetition of what may have already been said.

Mr. Swinburne has aimed at treating the subject of armature action in an alternator in what he regards as an unorthodox way, but which is really the orthodox way disguised. It is interesting to see to what this mode of procedure has led him, even although I do not in the least agree with his assumption that you cannot in the case of the armature of an alternator employ the ordinary notions of self-induction. Mr. Swinburne, in the early part of his paper, says: "The idea of self-induction is naturally inseparable from the notion of magnetic induction being inter-

Professor  
Ayrton.

“linked with the circuit.” Of course it is. “In an alternate dynamo the first definition”—that is, the definition just quoted—Mr. Swinburne goes on to say, “is meaningless, because there “is a field produced by the magnets; so, when the machine is “still, there may be a large number of lines of induction through “the armature coils without any armature current.” As well might one say that you cannot speak of the self-induction of any coil if it happens to be in the earth’s field, because lines of force may then be embraced by the coil other than those produced by the current in the coil. The self-induction of a coil is no doubt affected by the presence of iron in its neighbourhood, because the number of lines of force produced by a current flowing in the coil is altered by the presence or absence of iron. But to imagine, as Mr. Swinburne does, that the self-induction of a coil is “meaningless” because there may be a magnetic field about the coil, produced by some cause over which the current in the coil has no control, is a simple fallacy.

Since the reading of a paper recently by Mr. Taylor and myself at the Physical Society, we have been much criticised because our experiments on a transformer kindly lent us by Mr. Mordey showed that the loss of energy in the iron appeared to be less than would be obtained by calculating from the results published by Professor Ewing and Dr. Hopkinson for slow cycles of reversal of magnetisation. As the discussion of our paper has not yet been concluded at the Physical Society, I do not propose going at length into this matter now; but I should like to direct the attention of the meeting to a most interesting series of experiments on a Zipernowsky-Déri and Blathy transformer, carried out rather more than a year ago by Professor Roiti, of Florence, and a full account of which will be found in *La Lumière Electrique*, pages 479 and 525, vol xxxv., 1890. After giving the results of his measurements, Professor Roiti says: “But we are apparently thus led to a “deduction of considerable theoretical importance, and which “would be opposed to the generally accepted opinion of the “present day relatively to hysteresis in iron, and that is, that “the energy wasted on the reversals of the magnetism seems to “be negligible.

"This energy is usually calculated from data obtained from the remarkable researches of Ewing and Hopkinson, although the experiments of these investigators were made under conditions wholly different from those which exist in the working of transformers. Professor Ayrton.

"In these experiments, in fact, we start from a certain value of the magnetising force, which is made to vary slowly to another value, usually of the opposite sign, and then caused to return to the original value; whereas with transformers the reversals of the magnetism follow one another very rapidly, and very often between limits which become less and less separated, and nearer the point of inflection, the more the transformer is loaded. It is possible, then, to imagine that in such a case there exists some phenomenon analogous to that which is met with in the study of elasticity; at any rate, there remains a question which deserves to be studied in a thorough manner."

Mr. Mordey has mentioned this evening that the experiments made by my students on a Kapp and Snell transformer in 1888 led to a result similar to that obtained by our students last year when testing a Mordey transformer, and which, as I have pointed out, are in general accord with those of Professor Roiti's. It has always appeared to me questionable whether experiments made on *slow* cyclical changes of the magnetism of iron could afford data for the calculation of the loss of energy when the frequency of the cycles was 100 or 200 a second, and consequently I have always taught my students that the actual loss of energy in the iron of a transformer was rather a subject for experiment than for calculation from known data.

It may interest the meeting to know that Dr. Sumpner and I are communicating this week to the Royal Society a new method of measuring the power given by *any* current to *any* circuit. The method, as distinguished from the calorimetric method, is instantaneous; unlike the quadrant electrometer or wattmeter methods, it requires only the measuring instruments which are found in every electric light station; and, further, the correctness of this new method depends on no assumptions

Professor Ayrton regarding the nature of the current, or of the circuit the power given to which it is desired to measure.

It is my intention to apply the method to ascertain the exact shape of the characteristic curve of a "Hedgehog" transformer, to which Dr. Fleming has referred, and I would suggest to Mr. Swinburne that he should impress on his firm to supply the Central Institution with a highly efficient "Hedgehog" transformer, in compliance with the order already sent them. Whether the efficiency of the "Hedgehog" transformer be, as Mr. Swinburne states, 96.2 per cent. at full load, will then be ascertained; but we hardly require further experiments to tell us that the maximum efficiency of a closed-circuit transformer is not, as Mr. Swinburne states, limited to 92 per cent. at full load.

Mr. Trotter. Mr. TROTTER: The hysteresis may very probably disappear at high frequencies, and one is quite prepared to assume that it does. But the whole difficulty is, why does it diminish (the frequency being the same) with the increased load?

Mr. Swinburne. Mr. SWINBURNE: Instead of answering the various speakers, I will arrange my reply according to the sub-headings in my paper.

#### ARMATURE REACTIONS AND SELF-INDUCTION.

Several speakers have said that these are exactly the same thing, and Dr. Fleming has pointed out that, though the self-induction may vary throughout a period, its mean value can be taken. Professor Ayrton says that my statement that the armature reactions in an alternator cannot be treated as self-induction amounts to saying that a coil rotating in the earth's field cannot be treated as having self-induction. This is a complete misunderstanding of my armature reaction theory. According to it the field magnets are affected by the armature current, but the effect is due to a large number of periods, not to single half-periods. If a 2,000- and a 1,000-volt machine are coupled together and give 1,500 volts, the fields are affected. If armature reaction and self-induction were the same thing, the fields would be at their normal strengths at the instants when the

armature current is zero. This would mean that the induction in the fields would vary through an enormous range at a frequency of, say, 200 a second. This is, of course, impossible. The fields merely strike an average, and behave precisely as if the exciting current had been altered. The difference between the armature reaction and self-induction theory is not a question of variations of the value of the coefficient of self-induction throughout each period, though that variation itself might easily make the orthodox treatment useless for quantitative work.

Mr.  
Swinburne.

The difference between the theories may be best shown by mentioning some crucial experiments. According to the orthodox theory, an alternating dynamo with no field excitation will give no current when connected to a large condenser; according to the armature reaction theory, it will, if it is a small air space machine like the Ganz or Kingdon. Any other machine will also do it with a large enough condenser. I also believe that a dynamo with no field excitation will supply power to a motor whose field is excited, which is impossible according to the self-induction theory. These experiments have not been tried, so when tried they will be convincing proofs one way or the other, because they are suggested publicly before being made.

It has been said by Dr. Fleming that my paper adds nothing to what was shown in Mr. Mordey's paper. I agree with Mr. Esson, however, that that paper left the subject in a chaotic condition. Mr. Mordey performed certain experiments. He said they disproved the orthodox theory, and that if dynamos could have no self-induction they would run best in parallel. He carefully avoided bringing any theory forward. Dr. Hopkinson then pointed out that Mr. Mordey's experiments agreed absolutely with his theory of 1884. Sir W. Thomson, however, clearly referred to the armature reaction theory in his remarks. Dr. Fleming says that for parallel running the time constant should be small. He says it is small in the Ganz machines, which surprises me. He does not say why it should be small.

My position as to the orthodox theory is, that according to it, it is practically impossible to build station machines which will

Mr.  
Swinburne.

not run perfectly in parallel; and that, instead of the time constant being small, it may be varied between enormously wide limits. No one has criticised my calculations, so I may conclude either that the orthodox theory is wrong, or that there is no difficulty in running station machines in parallel, and never has been any. Dr. Hopkinson has said, "The machines will best control each other when  $\frac{2 \pi \gamma}{T}$ ,  $\gamma$  being the self-induction, is equal to the resistance of the armature circuit and the leads to the junction with the leads of the other machine;"\* and many people seem to take this as final for station dynamos. Dr. Hopkinson does not, however, say how he gets this value, or what he means by "best control." The only case I can think of in which that is the best value is that of running two similar machines with the least angular variation. This, however, is not a consideration of any importance in station work, and I have shown that the value of the self-induction may vary a thousand per cent. and the machines will still work perfectly in parallel, according to the orthodox theory. Mr. Esson says that I was wrong in supposing armatures have small self-induction when coreless. I was wrong in this.

#### SUB-STATIONS.

I have little to reply to under this heading. Major Cardew has explained that a transformer sub-station need not be an elaborate building, all that is needed being a lean-to shed. I would go further than this, and say no sub-stations are needed at all. At the end of a high-tension feeder, all that is needed is a hole in the ground with a transformer in it and a lid over it. It is generally assumed that a bank of transformers is needed, so that small ones can be used during the day. This is quite necessary with closed circuits, but large open-circuit transformers lose only about a quarter per cent. of the full-load power on no load, and it is no longer necessary to use automatic or other switches. Dr. Fleming says £1 a horse-power for large transformers is impossible. The price is taken from actual commercial

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\* *Journal*, 1889, p. 644.

work, and a little consideration of the cost of material and ease of manufacture of a "Hedgehog" will show why it is so low. Mr. Addenbrooke says that the London companies always intended to change to sub-station distribution with low-pressure service mains. I was engaged at the Board of Trade inquiry of 1889, and at the committees of both Houses, and remember nothing of the sort being brought forward then. I am very glad, however, to hear it now. Dr. Thompson has put the advantages of 1,000 volts better than I could.

Mr.  
Swinburne.

#### TRANSFORMERS.

Much of the discussion has been devoted to opposition to the "Hedgehog" transformer. The grounds have been that the "Hedgehog" is not so efficient as I have stated, that its exciting current renders it useless, and that closed-circuit transformers are more efficient than I allow.

The only attempt to make out that the loss on open circuit is large is by Mr. Kapp, who argues that a large exciting current must mean large waste of power, and that if the circuit is closed so as to reduce the exciting current less power must be taken. The large exciting current is made up of two parts. One is devoted to "magnetising" the air, if I may use such an expression; and the other—a very small component—is devoted to magnetising the iron. The first absorbs no power. Its instantaneous value is proportional to the rate of decrease of the pressure. If the pressure varied harmonically, this component would do so too, and it would lag  $90^\circ$ , and absorb no power. The small component, which magnetises the iron, also has its maximum value at the instant when the pressure is zero, but it absorbs power, as it is unsymmetrical. Its form can be got by making an "indicator diagram." An indicator diagram was worked out by me some twelve months ago,\* and since then Professor Ryan and others have made such diagrams by actual tests. Mr. Evershed has developed the idea thoroughly in the *Electrician* lately. Returning to Mr. Kapp's contention, if the circuit were closed with iron, the large component of the exciting current which takes no power

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\* *Industries*, December 20, 1889.

Mr.  
Swinburne.

would be done away with; but the small component would be doubled, and the power taken would be doubled. The exciting current would thus be enormously reduced, but the waste of power increased. For instance, with 2,000 volts and 0.3 ampere the loss in power is 17 watts, including the copper loss. If the circuit were closed with iron, like the old Wilde transformer, the exciting current would be about one-tenth of the present value, and you would have the form of closed-circuit transformer my firm would supply if asked—a form probably better than any now in the market. The efficiency at half-load for four hours a day would be reduced more than 6 per cent. by closing the circuit. Mr. Crompton says half-load for four hours a day is too much. With half-load for two hours a day the extra loss would be 12 per cent. People are so accustomed to losses in transformers that they hardly seem able to realise that it is even more serious than using low-efficiency dynamos.

Mr. Esson proposed to cut the transformer in two and make a link of it. He has made a slip in the length of iron needed, however. As there are flanges and insulation at the ends of the coils, there would be two coils  $8\frac{1}{2}$  inches long, instead of one  $13\frac{1}{2}$  inches. Using thinner flanges, we might say 8 inches. The coils are 7 inches diameter. The mean length of core is then 38 inches, instead of  $17\frac{1}{2}$  inches, or 120 per cent. more, instead of 66, as given by Mr. Esson. His proposal is not so good as Mr. Kapp's. Neither Mr. Kapp nor Mr. Esson would put in a dynamo giving, say, 86 per cent., if they could make one to give 95 per cent. at actually less cost.

Mr. Esson complains that I have not given particulars of my transformer; but, on the contrary, I have given every possible particular. He has the output and resistances, so that he can get the loss in the copper. He can also get the induction and waste of power in the iron. Mr. Esson uses hysteresis curves in dynamo work; they are just as applicable to transformers. Dr. Fleming says the efficiency cannot be a matter of calculation, but only of experiment. I can only reply that the loss in copper is known; and the loss in iron with known induction, frequency, and volume has been determined experimentally by

many observers. Dr. Fleming also says a wattmeter is not accurate. If you use a badly designed wattmeter, it is inaccurate; but it reads too high at no load, and too low at full load. If my wattmeter is bad, I have over-estimated the loss in iron in my transformers, though the readings at no load agree within a few per cent. with Ewing's data. There is no difficulty in making a good wattmeter. Mine has a circular coil of few turns, and very fine phosphor-bronze torsion wires. The self-induction can be calculated closely enough to tell if the instrument is one-tenth per cent. out. Professor Perry has recently published a convenient formula, which, however, I have not used. Sir W. Thomson has gone into the matter, and finds there is no difficulty in making a wattmeter accurate. I have not relied on calculations, however, but have carefully checked my wattmeters. Mr.  
Swinburne.

The next objection is the exciting current. It has been said that it lessens the output of the plant, or, according to a misuse of the word, the plant "efficiency." It has been assumed that I must run 30 per cent. of the engines all day. This objection is absurd. The exciting current can either be supplied by a condenser, so that one very small engine and dynamo will do day duty, or the small engine may drive one dynamo, and two others may run round loose, the engines being disconnected. In either case, of course, much less engine-power is needed than with the closed circuit. Dr. Fleming says I omit the consideration of "drop." The drop in the "Hedgehog" is much less than that in the closed-circuit transformers of which I have been able to get particulars. For instance, the drop in a 200-light or 6,000-watt "Hedgehog" is just under 2 per cent. Mr. Mordey gives the loss by copper alone in a transformer of the same size as 176 watts, or 2.85 per cent. If the inductive drop is equal to the copper drop, which is generally the case in commercial transformers, this would mean  $5\frac{1}{2}$  per cent. I cannot give the inductive drop in the Mordey transformer, as he does not give the particulars of windings. In a 1,000-volt 1,500-watt transformer, illustrated in Dr. Thompson's "Dynamo-electric Machinery," p. 499, the total drop comes out 6.47 per cent.

Mr.  
Swinburne.

The next objection is that the closed-circuit transformer I took was not a fair sample. I repeatedly asked for an actual transformer which is better, but none was forthcoming. The transformers which have most sale in this country have, on the average, more iron than that taken by me. From a patent drawing I have seen, I think Mr. Kapp's are better, but I have not seen any of his recent transformers. I do not for a moment say that a closed circuit cannot be made better than my example, but that they are not. Mr. Evershed says I ought to design a special closed-circuit transformer to compare. In making commercial transformers you do not alone consider efficiency at some particular load; but you make a compromise among conflicting desiderata. You have to consider efficiency at various loads, drop, cost of copper, cost of labour, cost of iron, mechanical strength, insulation, cheapness of manufacture, and weight. Closed-circuit people have had six years to improve; I have had eighteen months. I therefore take, not a theoretical, but an actual commercial transformer, and compare it with a typical commercial closed-circuit transformer. Mr. Evershed says the transformer should have 2,000 primary turns, not 400. If he will try and pack these into 2 inches by 1 inch—the space in a Mordey transformer—he will find it impossible.

Though not called upon to design closed-circuit transformers, I have already done so, and my B.A. paper of 1889 was the first to show how closed-circuit transformers should be designed for all-day loads; and the ungraceful way in which closed-circuit people utilise my work, without the least acknowledgment, does not encourage me to publish designs for more closed-circuit transformers.

The next assertion of some of my opponents is that the loss in iron at light loads is less in a transformer than that given by Ewing, Hopkinson, and others. It is urged that the loss as taken by a ballistic galvanometer is no guide, because the change of induction is then slow. If this were so, and there were a time lag, the loss in iron would be greater at high frequencies, not less, so that would be against closed-circuit

transformers, not in their favour. The statement is erroneous, <sup>Mr. Swinburne.</sup> however; the change of induction that affects the ballistic galvanometer is probably just as rapid as, or more rapid than, in the case of a transformer. When the ballistic galvanometer is used as described by Messrs. Evershed and Vignoles during the discussion on the Presidential Address last year, the sudden changes in the extreme readings are between the extreme limits of the variation of the induction, and correspond exactly to the energy taken during very rapid reversals. Mr. Evershed finds most commercial soft irons are a little worse than Ewing's sample. Mr. W. Fox-Bourne and I have, however, made careful determinations of the loss in iron magnetised by ordinary alternating currents.\* We find most samples take more power than Ewing's, though one took a little less. There is thus no doubt that the losses in iron are quite as great as those given by Ewing, and an allowance must be made for Foucault currents. Such data are always used in design of dynamos, and must be applied also to transformers.

It has been said, also, that the loss in iron decreases as the load increases. I hardly think such a suggestion can be taken seriously. It matters little in transformer design, as the full-load efficiency of a transformer is of no importance. I have been accused of putting important transformer tests aside lightly. I put them aside because I had studied them carefully, and anyone else who studies them carefully will also put them aside. With the exception of some American tests, they are quite valueless. The first is that of Professor Ferraris. He used a split dynamometer. A split dynamometer does not measure the power taken by the primary circuit. It under-estimates it. If the full-load loss in iron is found by measuring the power absorbed by the primary, which is large—say 100—and deducting the secondary output, which is also large—say 92—and the loss in copper, which is, say, 6 in this case, the result is probably wrong. A very small percentage error in the measurements may get the iron loss out — 2 instead of 2. Such errors must occur, when to errors of

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\* "Testing Iron," B.A., 1890.

Mr.  
Swinburne.

observation you add errors due to methods which cannot measure the primary power. Mr. Mordey suggests that when the iron loss is very small a small error might make it come out negative. Ferraris's test makes its negative value as great as its maximum positive value. I quite agree with Mr. Mordey that to attempt to determine hysteresis loss at full load by difference is useless. The freezing mixture transformer was, by the way, an old form of open circuit. Professor Ayrton's recent experiments were carried out by the split dynamometer method, and the full-load efficiencies are therefore far too high. Professor Ayrton holds that a wattmeter is very inaccurate; but the split dynamometer includes all the errors of the wattmeter, with those introduced by the waste field of the transformer. In addition to this, I must repeat that to try to determine a loss alleged to be under one-tenth per cent. by difference is absurd. No engineer who wanted to find out the consumption of, say, a brake-pump on a locomotive, would measure the evaporation when the engine was standing still, and when running, and then indicate the engine itself when running, and find the loss by difference. If he did, he would as likely as not find the brake-pump took no power when the engine was running. He would simply measure the steam taken by the pump when the engine was standing, and he would naturally assume that it took the same power when the train was running. Professor E. Thomson or Mr. Rice, and Mr. Stanley have also recently made tests with the split dynamometer. They also doubtless give too high full-load efficiencies.

The tests of Professor Ryan and Messrs. Merritt and Humphrey and Powell deserve more careful consideration, as they have been taken with great care, and no unwarrantable assumptions have been made. They get the loss in iron by difference, however. At the time of their publication, I pointed out that some of the curves were evidently misplaced; and my criticisms were treated very frankly. The least error in the position of the contact, when a half-period is really only an angle of  $22\frac{1}{2}^{\circ}$  or  $36^{\circ}$ , and the arm with the contact is only two or three inches long, may throw a value got by difference entirely wrong.

Mr. Esson refers me to some experiments of Professor Ayrton's in 1888. Professor Ayrton put the transformer in a water jacket, and allowed water to run through the jacket. He allowed a maximum of three hours for the issuing water to come to its steady temperature. As Mr. Crompton has pointed out, a transformer takes ten times as long as that to come to its steady temperature in an ordinary room. In a calorimeter where there are no draughts, it takes longer. An ordinary voltmeter does not reach its final temperature in four hours. Professor Ayrton's determinations of the losses are thus ever so much too small; moreover, there appears to be no correction for radiation. His readings are otherwise inaccurate. He tried a wattmeter; not a special instrument, but one, no doubt, useless for alternating currents. He got readings in some sense 9 per cent. larger than the products of the effective volts and amperes, which is impossible with any wattmeter. He rejects both the wattmeter and electrometer readings, presumably because they do not agree with the calorimeter, and says that the laws of electrometers—that is to say, the fundamental laws of electrostatics—are wrong.

I have not thoroughly considered Professor Roiti's tests. He uses the same calorimeter method as Professor Ayrton, though he allows four hours. He also concludes there is something wrong with the laws of electrostatic wattmeters, because they read more power than his calorimeter. No doubt the error is entirely in the calorimeter. Mr. Mordey has made some very rough tests himself, and he allows eight hours. Mr. Crompton's and my own experience show that that is far too little, and the method of testing seems far too rough to give any information. The mere fact that the readings agree at all with those taken by a split dynamometer, which are necessarily largely wrong, shows that Mr. Mordey's tests are wrong too.

My position is, therefore, that there may be reduction of hysteresis at large loads, but that there is no evidence in support of the contention. Moreover, putting on load does nothing to the iron. The increased ampere-turns in the primary are exactly neutralised by those in the secondary. This state-

Mr.  
Swinburne.

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ment must be qualified to the extent that the loss by primary resistance and "drop" renders a slightly smaller back E.M.F. necessary, so at full load the induction is very slightly diminished.

I do not call attention to the errors in these tests in any carping spirit, but because they are especially likely to mislead on account of the position of the experimenters. It is most important that the large coal bills of central stations should be ascribed to the true cause, so that such faults, being first traced, can be remedied.

As already mentioned, decrease of hysteresis at full load is not to the point in all-day transformers. It would be very important in dynamos. Iron is now put into alternators on sufferance only, as its advantage in making a solid mechanical armature is balanced by the loss by hysteresis. If this did not exist, such machines as those of Messrs. Ferranti, Mordey, Siemens, and Brush would be the very worst designs. Armatures should have their conductors solidly attached to a strong iron core in a mechanical way. High inductions should then also be used in all dynamos which generally work at large loads.

I have been accused of inconsistency by Mr. Mordey because I corrected a statement of Professor Forbes's as to loss in hysteresis,  $\int I dH$ , in an open-circuit transformer being less than in a closed. Mr. Mordey has quoted this without realising that  $\int I dH$  is the loss per cubic centimetre per cycle, not the power in the core. This loss is the same for the same induction in both cases. Mr. Tesla has referred to a back statement of mine that a particular form of open circuit is bad. So it is; I never advocated all forms of open circuit. Bringing up such things is waste of time, and serves no scientific purpose. Both statements were correct. If not, it would not matter; consistency of opinion is not a virtue. I hope to go on changing my opinion as I learn.

I cannot well answer Mr. Fricker, as his diagrams are not now before me. If the dynamo gave a harmonic curve, the induction would also be harmonic; Mr. Kapp therefore draws a circle, or a rotating line, for it. The projection of this line, gives, of course, the instantaneous induction. If there is no magnetic leakage, the

primary E.M.F. and secondary E.M.F. and secondary current are all in quadrature with this. There is no begging the question in this. Electro-motive force is the rate of increase of induction, by its fundamental definition. The transformed primary current is also in quadrature with the induction, and in step with the primary E.M.F. The magnetising current cannot be shown on such a diagram except as a line in step with the induction, but varying in length, so that its length is proportional to  $H/B$ . The projection of this must be added to that of the active primary current to give the full instantaneous primary current. If there is magnetic leakage, the active primary current lags after the primary E.M.F. The secondary E.M.F. also lags after the primary E.M.F., and so does the secondary current. The secondary E.M.F. can never be in quadrature with the primary current if there is a secondary current; and I can hardly think Mr. Blakesley ever said so. He may have assumed  $H/B$  to be constant, so that the magnetising current varies harmonically: at no load the secondary E.M.F. is then in quadrature with the primary current. As  $H/B$  is never constant, such expressions as "in quadrature with," or "angle of lag" of the exciting current are meaningless. I do not quite follow Mr. Fricker's treatment of periodic functions, but think he has forgotten that the terms of a Fourier's series have different frequencies, so that they cannot be separated and dealt with, and added together again after the operation, as Mr. Fricker proposes.

A ballot for new members took place, at which the following candidates were elected:—

*Associates:*

Huibert Doijer.		Arthur Handley.
John Edwin Stewart.		

*Students:*

Gilbert Fox Allom.		Alfred George Newington.
Frederick W. Forrest.		Wilfred Morgan Oliver.
Richard Percy Lovell.		Henry George Shoolbred.

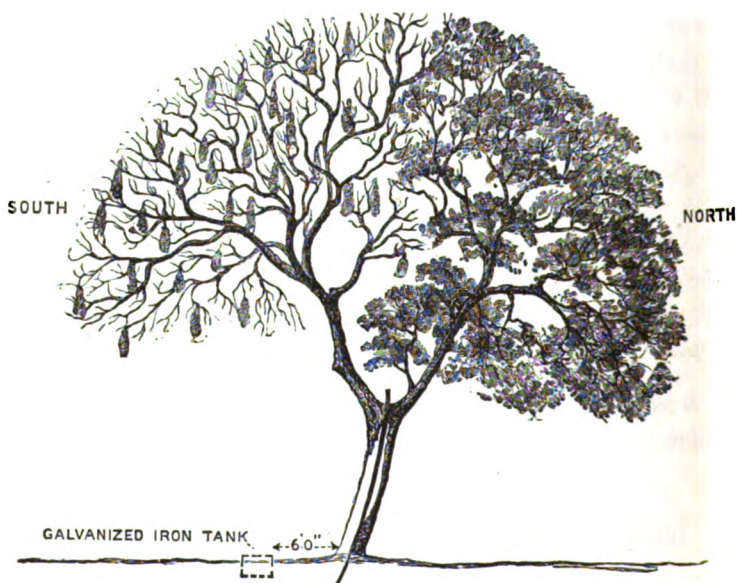
The meeting then adjourned.

## ORIGINAL COMMUNICATION.

### COPY OF NOTE BY THE ELECTRICIAN, INDIAN TELEGRAPH DEPARTMENT.

*(Communicated by the Director-General of Telegraphs, India.)*

A severe thunderstorm passed over Thayetmyo, in Lower Burma, on the evening of the 27th April, 1890. It is reported that one flash of lightning was exceptionally vivid, and appeared



Elevation of a Neem Tree struck by Lightning on the night of the 27th April, 1890, at about 9.30 p.m., near P.W.D. Office, Thayetmyo.

to last for several seconds. The flash was immediately followed by a terrific peal of thunder, which shook the building in which the telegraph office is located. The following day it was found

that a Neem tree, about one-quarter of a mile from the telegraph office, had been struck by lightning, and it is conjectured that the damage was done by the particular flash above alluded to. On an inspection of the tree and its surroundings, it was found that at the top of the trunk of the tree the two main branches diverge, and form with each other a curve at the point of diversion. About 6 feet from the base of the tree, a zinc tub, measuring 5 ft. by 2 ft. by  $2\frac{1}{2}$  ft., about, is buried.

On visiting the tree for the first time, it was surmised that the curve previously alluded to was the point of attraction for the electric discharge, inasmuch as the top of the tree remained covered with thick foliage, which retained its freshness and colour for some days after the occurrence of the storm. Subsequently, however, on the occasion of the second visit, it was noticed that the branches on the exposed side of the trunk had withered, and that decay had already commenced over half the trunk; the remaining portion of the tree, however, still retaining its usually healthy appearance. The trunk, from the middle of the curve, was found split in two right down to the ground, and the bark on the damaged side completely torn away, in evenly divided shreds, and resting on the ground. The operation was of so fine a nature that it is doubtful if it could have been better done by the most dexterous handicraft. The wood of the trunk on its exposed side presented a perfectly smooth surface. It appears strange that this particular tree should have been struck, seeing that many others of greater height, and also on a higher ground, exist within a few feet of the injured one; and it is a question, therefore, whether the zinc tub alluded to above was the primary cause of the disruptive discharge taking place where it did. A drawing of the damaged tree accompanies this.

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## ABSTRACTS.

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### **E. LÉVAY**—COMPARISON OF THE TOTAL ELECTRICAL WORK DONE BY A CELL WITH THE TOTAL CHEMICAL ENERGY EXPENDED.

(*Wiedemann's Annalen*, Vol. 42, p. 103.)

The author employed a small cell circuited through a silver voltameter, both of special construction. Having weighed the plates of both, the cell and voltameter were placed in a calorimeter, and the heat developed during half an hour's run, determined; many precautions being taken to prevent errors. The plates were then again weighed. The cells experimented on were a Daniell, with copper-sulphate solution  $\Delta$  1.174, and zinc-sulphate  $\Delta$  1.095; and De la Rue cells, with 1, 2, and 4 per cent. solutions of zinc chloride. The results show that, with the Daniell, the electrical work done exceeds the chemical energy expended by about .8 per cent.; or, in other words, that the cell absorbs heat when working. In the De la Rue cells the reverse obtains, the cell giving out from 5 to 9 per cent. of the total chemical energy as heat. The results compare well with those of Jahn, but the author claims that his own are probably the more accurate of the two, as the whole of his circuit was enclosed in the calorimeter.

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### **E. BLATTNER**—OPTICAL EFFICIENCY AND WASTE HEAT OF GLOW LAMPS.

(*Beiblätter*, Vol. 14, p. 1172.)

The lamp to be tested was placed in a copper vessel full of water and run for a given time; it was then placed in an exactly similar glass vessel and run at the same current for the same time. The rise of temperature of the water at the end of the run was taken in each case; the rise in the copper vessel being proportional to the whole work done in the lamp, and that in the glass one to the same, minus the visible radiation. The ratio of the difference between the two, to the rise in the copper one, was taken as the efficiency of the lamp. The tests, which were taken some years back, show that (at that date) when running at normal candle-power the efficiency of commercial lamps was about 5 to 6 per cent.; when very much over-run, an efficiency of 10 per cent. could be obtained.

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### **J. TROWBRIDGE**—MOTIONS OF ATOMS IN THE VACUUM DISCHARGE.

(*Phil. Mag.*, Vol. 30, p. 480.)

This is a spectroscopic investigation of the question whether in the vacuum discharge the atoms of the metals of the terminals between which it passes are conveyed to and fro by the discharge or not.

It is shown that if they were so conveyed their velocity would be 5,000 metres per second in one experiment tried, and 40,000 metres in another. These velocities, if they existed, would cause a very perceptible broadening of the lines in the spectrum when the spectroscope was end-on to the line of motion. The oscillating spark passed between two iron electrodes; one of these was hollow, and through it a photograph of the spectrum was taken. Another photograph was then taken with the direction of the discharge across the slit, but on comparing it with the former, or end-on, one, the breadth of the lines was identical.

The conclusion, says the author, seems to be a strong one that electrical oscillations do not carry the atoms of the metals with them in spark discharges. The atom is merely shaken up and caused to emit the vibrations which appeal to our senses as light and heat, while the electrical waves pass on without conveying the atoms.

In reference to the above, Herren Wiedemann and Ebert call attention (*Phil. Mag.*, vol. xxxi, p. 288) to experiments of their own, which they claim to be 200 times as sensitive as those of Professor Trowbridge, and which also fail to show any broadening of the lines.

### **M. ASCOLI—THE CONNECTION BETWEEN THE ELASTICITY AND ELECTRICAL RESISTANCE OF METALS.**

(*Beiblätter*, Vol. 14, p. 1124.)

Wires of platinum, silver, and iron were experimented on. Their resistances and elasticities were measured before and after being heated in an air bath. The results show that a change in the latter property is always accompanied by a change in the former, and that in general resistance diminishes with increase of elasticity.

### **LE CHATELIER—EFFECTS OF HEATING ON THE RESISTANCE OF STEEL.**

(*Comptes Rendus*, Vol. 112, p. 40; *La Lumière Electrique*, Vol. 39, p. 145.)

The process of hardening affects both the chemical condition of steel and its internal structure, and it is desirable to separate the effects due to the one from those caused by the other. With this end in view, the author publishes some tests on the variation of resistance of steel with varying temper.

**HARDENING.**—A considerable increase of resistance occurs (13% up to 60%) when steel is heated to 730° and then hardened, but not if the temperature does not reach that limit; nor is the increase of resistance greater if a higher temperature is employed. This confirms one point in Osmond's theory, viz., that "positive" hardening (that is, hardening accompanied by brittleness) has the effect of retaining in the steel at ordinary temperatures that molecular structure which is otherwise only stable at temperatures above 730°. At the same time, it is opposed to his statement that hardening retains part of the metal in the "β" state—i.e., the molecular condition usually only stable above 850°—inasmuch as higher hardening temperature than 730° has no effect on the resistance.

**ANNEALING.**—This reduces the resistance of the hardened metal, the effect being greater the longer the metal is kept at the annealing temperature. There appears to be a certain definite reduction of resistance which would be obtained with each particular temperature if continued for an infinite time, and which would not be exceeded; but in every case the greater part of the reduction takes place in a very short time.

**EFFECT OF THE TEMPERATURE OF THE ANNEALING BATH.**—Steel wires 2 mm. (14 S.W.G.) were raised to hardening heat and then plunged into baths varying from 100° to 450°, and measurements of their resistances made at once, and continued for some time. They were found to reach the temperature of the bath in a very few seconds. Their resistance then remained constant for a time, varying from some seconds to several minutes, depending on the nature of the wire and the temperature of the bath. They then began to anneal suddenly, the resistance falling rapidly at first, and then more slowly, the final state being the same as if annealed in the same bath after having been hardened in a cold one; but when treated in this latter way, no such “retardation” of annealing could be observed.

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#### **E. MASSIN—TESTS ON CAPACITY AND MUTUAL AND SELF INDUCTION ON OVERHEAD WIRES.**

(*Annales Télégraphiques*, Vol. 17, p. 499.)

It was found to be impracticable to take tests on most lines, as the currents on neighbouring wires interfered with the results. Two wires were, however, obtained between Epernay and Montmort which were undisturbed at night.

The wires were No. 11 S.W.G. iron, 16 inches apart, about 15 feet from the ground, and about 11 miles long. The insulation of wire *a* was 330,000  $\omega$ , that of *b* 470,000  $\omega$ . Looped at the far end, their resistance was 654  $\omega$ . The low insulation resistance made a capacity test difficult, but an approximation was obtained. Capacity of a single line, one battery pole to line and the other to earth, .015 microfarad per mile; and with one pole to one line and the other to the other, .012 microfarad per mile.

The mutual induction between the two wires was .0051 secohms per mile; the inductance of the two looped, .019 secohms per mile. The above results are to be taken as approximate only.

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#### **GRAWINKEL and STRECKER—THE USE OF ACCUMULATORS IN THE BERLIN G.P.O.**

(*Elektrotechnische Zeitschrift*, Vol. 11, p. 629.)

Three separate batteries are used, each of 40 cells, each cell having three positive and four negative plates, and a capacity of 10 amperes for 5.2 hours. Two sets are joined in series for discharging, while the third is charged from the town lighting mains. This, however, is only on account of the lighting company forbidding the mains to be earthed; as, in the preliminary tests, when a dynamo belonging to the Department was used, the charging was not found to interfere with the working of the instruments. One end of the double set being put to

earth, a connection is made to every tenth cell, thus giving circuits for 20, 40, &c., up to 160 volts. In the 20-volt circuit a resistance of  $20\ \omega$  is inserted, in the 40-volt one of  $40\ \omega$ , and similarly for the others, so that in case of a short-circuit the current cannot exceed one ampere. In the earth wire there is a low-resistance relay which rings a bell when the total current in all the instruments rises to about .5 ampere, which should only occur when giving the morning time signal on all the instruments together. The maximum working current used does not otherwise exceed .4 ampere, and throughout the day averages .2 to .3 ampere.

The exchange of sets from charging to discharging is done by a rotary commutator, which at the same time joins up the cells to their proper circuits. It is done without interfering with the working of the instruments. The 160-volt circuit replaces the largest battery previously used—200 copper-sulphate cells. Altogether 93 Hughes and 144 long-distance Morse circuits are worked, the total number of copper-sulphate cells replaced by the accumulators being 6,000. The commutator and other arrangements are described at some length, and illustrated.

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#### **E. ELSAS**—THE USE OF THE "DIFFERENTIAL INDUCTOR" AND TELEPHONE FOR RESISTANCE MEASUREMENTS.

(*Wiedemann's Annalen*, Vol. 42, p. 165.)

The differential inductor used resembled a Rhumkorff coil. Core, 4 in.  $\times$   $\frac{1}{2}$  in.; primary, 600 turns 22 S.W.G; secondary, two exactly similar wires wound together, each 5,200 turns 38 S.W.G. There is a mercury contact-breaker in the primary circuit. The instrument is intended for measurements of electrolytes and other non-inductive resistances, either by balancing them against a resistance box or using a slide wire bridge. The simplest connection is to join the two inner ends of the secondary together, and the two outer ends through the unknown resistance in series with a resistance box; the two outer ends are also joined through a telephone, and the resistance box altered till silence is obtained.

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#### **GRAWINKEL** and **STRECKER**—APPARATUS FOR MEASURING WAVES OF ELECTRICAL CURRENT.

(*Elektrotechnische Zeitschrift*, Vol. 12, p. 6.)

The authors employed a spindle on which was a disc of ebonite, with contact-pieces so placed on its periphery that when the spindle was rotated the pieces made contact with fixed brushes, and thus a series of long or short currents was sent through a telegraph line or other circuit. In the authors' case the letter *f* (- - -) was used. A resistance of negligible capacity and inductance was inserted in the circuit, and by means of another small contact-piece on the rotating spindle the ends of the resistance were joined to the terminals of a galvanometer during a very small fraction of each revolution; the fixed brush which this contact-piece touched could be set in any desired position, so that the E.M.F. at the terminals of the resistance, and thus the current in the circuit, could be determined at various times during the progress of a dot or dash. This method of working, however, is liable to introduce errors, owing to the contact with the

brush not always lasting for the same time. The authors, therefore, prefer to replace the galvanometer by a condenser, which is left on for a considerable number of revolutions and then discharged through a ballistic galvanometer; or an electrometer may be used instead of the condenser and galvanometer. An arrangement is also described by which only one current is sent per revolution, and its duration altered without stopping the spindle. A half-horse-power motor was used for driving, and the maximum speed was from 30 to 40 revolutions per second. Numerous curves were taken at 20 revolutions per second.

### **Dr. G. MENGARINI—ELECTROLYSIS BY ALTERNATING CURRENTS.**

(*La Lumière Electrique*, Vol. 38, p. 541.)

The author considers that no new hypothesis is required to account for all observed results in electrolysis by alternating currents. A voltameter in an alternating circuit acts in the same way as a conductor possessing inductance. When there is no electrolysis the current follows a sine law, and so does the E.M.F. of polarisation, and the P.D. at the voltameter terminals, but they are not in the same phase. If the polarisation becomes so great that electrolysis takes place, the upper part of the back E.M.F. curve becomes a straight line. The following are given as the results of experiment:—In electrolysis of acidulated water or saline solutions, the evolution of gas at one electrode can be partly or wholly stopped by increasing its surface, and thus diminishing the current-density per unit area of plate, and consequently the polarisation. This superposes a direct current on the alternating one, and the effect may be carried so far as to "redress" the current to a unidirectional one. During electrolysis the electrodes are much corroded, even when of gold or platinum. When using either of the latter metals, considerable polarisation is found to exist after the current is stopped.

### **G. ROUX—INDICATOR FOR THE AMOUNT OF CHARGE IN AN ACCUMULATOR.**

(*Bulletin Soc. Int. Elect.*, Vol. 7, p. 390.)

Starting on the basis that the mean density of the electrolyte varies directly as the amount of charge, the author immerses in a discharged cell a hollow cylinder of glass, nearly as long as the liquid is deep, and weighted at the lower end so as to stand on the bottom. This he attaches, by a fine platinum wire, to a small lever on a spindle pivoted horizontally above the cell, and furnished with a counterbalanced pointer. By means of an adjustable counter-weight on the spindle, the weight of the cylinder, in the discharged cell, is exactly balanced when the pointer is at zero. As the cell is charged the weight of the cylinder in the liquid becomes less, and it rises somewhat, thus deflecting the pointer. When the cell is fully charged the deflection of the pointer is, by a second counter-weight, adjusted to  $45^\circ$ , and then reads 100 on a tangent scale; the indications on this scale now give directly the percentage of full charge in the cell. The two counter-weights are small brass bobs, which can be screwed nearer to or further from the axis. The pins on which they run are set at  $45^\circ$  to one another; when the pointer is at zero the weight for

adjusting the full deflection hangs vertical, and, similarly, the zero-adjusting weight is vertical when the pointer is at full deflection. The two points are therefore set independently of each other. It will be noticed that from the form of the float its indications will be proportional to the mean density, and not, as in most forms, to the density of a particular layer of the liquid.

### **E. PIAZZOLI**—COUPLING ALTERNATORS IN PARALLEL.

(*La Lumière Electrique*, Vol. 38, p. 481.)

This is a short description of the method employed by Messrs. Ganz & Co. (Zipernowski system). To join up a new machine with one or more already running, it is first put on to a load of lamps, and also on to one primary of a three-circuit transformer, another primary circuit of which is on to the other dynamo. The secondary coil of the transformer has a lamp on it which lights steadily as soon as the two machines synchronise. A diagram of the dynamo circuits is given.

### **HILLAIRET**—EFFECTS OF LIGHTNING ON A TRANSMISSION OF POWER LINE.

(*Annales Télégraphiques*, Vol. 17, p. 558.)

The line is one erected by the author at Domène (Isère). It is 5 kilometres long, and lies in a valley where violent thunderstorms are frequent. The machines are, in consequence, very carefully insulated (R to earth about 100,000  $\omega$ ), and each end of the line has a lightning-guard, of a magnified telegraph pattern, with combs half a metre long. The two lines are carried on 130 poles, on insulators. Below them is a telephone line. Last May the line was struck; 19 poles, all consecutive ones, being split up. These were at a part of the line where there is a bed of clay a little below the surface. The most curious point is that none of the posts were struck at the top, but all at the level of the telephone wire. The marks left by the discharge began on all the poles on the windward side, where the rain had been beating. Only one insulator was broken. At every flash during the storm there was a shower of sparks at the brushes of the machines, but they sustained no damage; and although there were heavy discharges at the lightning-guards, the points of the latter remained quite sharp and uninjured.

### **E. MERCADIER**—NOTE ON THE LOUDNESS OF THE TELEPHONE.

(*Comptes Rendus*, Vol. 112, p. 96.)

The author has already shown (*C. R.*, 8-15, April, 1889) that, for any telephone of given magnetic field, there is a certain thickness of diaphragm which gives the greatest intensity of sound. He now points out that, theoretically at any rate, there is also a diameter of diaphragm which should give the best results, and that it should increase with the strength of the magnetic field. In a given telephone the effect of increase of strength of field is not so great as might have been expected. Replacing the permanent by an electro-magnet, the current round which was slowly increased, the author found that a point was soon reached beyond which further increase was of no benefit.

# LIST OF ARTICLES

## RELATING TO

# ELECTRICITY AND MAGNETISM

Appearing in some of the principal Technical Journals during the Months  
of FEBRUARY and MARCH, 1891.

### BATTERIES.

- F. ROSS—Accumulators (Paper before the El. Tech. Ver., and Discussion).—*El. Zeit.*, vol. 12, p. 91.  
—MALATERRA—The De Méritens Primary Cell.—*Lum. El.*, vol. 39, p. 470.

### LIGHT AND POWER.

- ANON.—The Frager Meter (illustrated).—*Lum. El.*, vol. 39, p. 481.  
ANON.—The Oerlikon High-Tension Experiments.—*Lum. El.*, vol. 39, p. 282.  
L. BAUMGARDT—Analytical and Graphical Methods in Dynamo Design (Paper before the El. Tech. Ver., and Discussion).—*El. Zeit.*, vol. 12, p. 80.  
J. BOIS—Graphic Construction for rapidly Calculating most Economical Size of Leads.—*Lum. El.*, vol. 38, p. 375.  
F. GERALDY—The Electric Lighting of Paris.—*Lum. El.*, vol. 39 (series of articles).  
E. HUET—Official Report on the Working of the Halles Centrales Installation.—*Lum. El.*, vol. 39, p. 433.  
G. RICHARD—Train Lighting.—*Lum. El.*, vol. 39, p. 367; and sundry previous and subsequent numbers.  
G. SCIAMMA—Electric Lighting of the Stations of the Compagnie du Nord.—*Bull. Soc. Int. El.*, vol. 8, p. 6.  
J. SWINBURNE—Alternate-Current Condensers (Physical Society).—*Phil. Mag.*, No. 189, p. 102.  
G. RICHARD—Electrical Welding.—*Lum. El.*, vol. 39 (series of articles).

### ELECTRO-CHEMISTRY AND ELECTRO-METALLURGY.

- F. BRAUN—Electrolysis.—*Wied. Ann.*, vol. 42, p. 450.  
A. MINET—Electro-Metallurgy of Aluminium.—*C. R.*, vol. 112, p. 231.

### TELEGRAPH AND TELEPHONE.

- ANON.—Underground Telegraph and Telephone Wires in Towns.—*Lum. El.*, vol. 39, p. 471.  
A. FRANKE—Tests on the Wave Form of Telegraphic Signals on various Lines (Paper before the El. Tech. Ver.).—*El. Zeit.*, vol. 12, p. 108.

- GRAWINKEL and STRECKER—The Use of Small Accumulators in Parallel with Daniell's for Heavy Country Telegraph Work.—*El. Zeit.*, vol. 12, p. 129.
- LANDRATH—The Underground Lines of the Berlin Telephone Service (Paper before the El. Tech. Ver.).—*El. Zeit.*, vol. 12, Nos. 4, 5, 6.
- E. MERCADIER—Telephonic Reproduction of Speech.—*C. R.*, vol. 112, p. 156.
- J. B. POMEY—Claude's Relay.—*Ann. Tel.*, vol. 18, p. 54.
- WILLOT—Willot's Relay.—*Ann. Tel.*, vol. 18, p. 63.

### MAGNETISM

- P. BACHMETJEW—Relation between Magnetic Permeability and Atomic Weight. (Exner Rep., p. 557, 1890.)—*Beibl.*, vol. 15, p. 123.
- S. BIDWELL—Lecture Experiment Illustrating the Effect of Heat upon the Magnetic Susceptibility of Nickel.—*Phil. Mag.*, No. 189, p. 136.
- M. CANTONE—Change of Form of Paramagnetic Substances in a Magnetic Field.—*Rend. Lincei*, p. 252, 1890; *Beibl.*, vol. 15, p. 49.
- MOUREAUX — Magnetic Disturbance accompanying the Earthquake of January 15 in Algeria.—*C. R.*, vol. 112, p. 259.
- H. NAGAOKA—Momentary Currents caused by Twisting Magnetised Wires of Iron, &c.—*Jour. Col. Sci. Japan*, p. 335, 1890; *Beibl.*, vol. 15, p. 53.
- A. PALAZ—The Electro-Magnet.—*Lum. El.*, vol. 39 (series of articles).
- S. T. PRESTON—The Problem of the Behaviour of the Magnetic Field about a Revolving Magnet.—*Phil. Mag.*, No. 189, p. 100.
- A. SCHUSTER—Influence of the Bending of Magnetic Needles on Apparent Magnetic Dip.—*Phil. Mag.*, vol. 31, p. 275.
- Prof. F. J. SMITH—Measurement of Time of Fall of Magnetisation of an Iron Cylinder.—*Phil. Mag.*, vol. 31, p. 64.
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# JOURNAL

OF THE

## Institution of Electrical Engineers.

*Founded 1871. Incorporated 1883.*

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The Two Hundred and Twentieth Ordinary General Meeting of the Institution was held at the Institution of Civil Engineers, 25, Great George Street, Westminster, on Thursday evening, April 9th, 1891—Mr. WILLIAM CROOKES, F.R.S., President, in the Chair.

The Minutes of the Ordinary General Meeting held on March 12th were read and approved.

The names of new candidates for election into the Institution were announced and ordered to be suspended.

The following transfers were announced as having been approved by the Council:—

From the class of Associates to that of Members—

Frederick Francis Bennett. | Herbert Laws Webb.

From the class of Students to that of Associates—

J. H. Stephens. | Alfred Sykes.

John Frederick Tester.

Donations to the Library were announced as having been received from Messrs. Baillière et fils, publishers; Thos. L. Miller, Esq.; Arthur Cowper Ranyard, Esq.; A. R. Bennett, K. L. Murray, Professor Henry Robinson, and Charles Todd (Astronomer Royal, New South Wales), Members; to whom the thanks of the meeting were duly accorded.

The SECRETARY: I have to announce another donation, which I have laid on the table for the inspection of members, and

which is of great interest, especially to telegraph men. I will read the few lines that came with it from our Associate Mr. Frank A. Bailey, Inspector of Telegraphs, Perth, Western Australia.

WESTERN AUSTRALIA, PERTH,

12th February, 1891.

DEAR SIR,—By accompanying mail you will receive a small parcel containing two spear-heads made by the natives of the Kimberley district, W.A., through which our Derby Halls Creek and Wyndham line runs: they are made out of *Cordeaux's* insulators. I have had some difficulty in getting them for the Society, as the natives are not only shy, but keep aloof from the whites. You will observe one is already set. The spear from which it was cut off measured fully 9 feet—somewhat a formidable weapon. I, for one, should be very sorry to receive it, as no doubt the leakage would be considerable! It has already come some 1,700 miles through the post, so I trust it will reach you safe, and prove of some interest to your collection.—I am, Sir, yours faithfully,

FRANK A. BAILEY,

*Inspector of Telegraphs, W.A., A.I.E.E.*

F. H. WEBB, Esq.,

*Secy. I.E.E.,*

London.

P.S.—The gum used with the spear is Xanthorrhiza.

F. A. B.

The PRESIDENT: This is a very remarkable instance of the ingenuity of the natives in making spear-heads out of so hard and brittle a substance as porcelain. I have much pleasure in proposing a vote of thanks to Mr. Bailey for his kind gift, which is additionally welcome as proving the interest taken in the Institution by our members in the Antipodes.

The vote was carried by acclamation.

The PRESIDENT: The next matter is the Balance-Sheet for the year 1890, of which you have all received a printed copy, and I assume that we may therefore consider it as read. We must all feel gratified that the finances of the Institution are in such a satisfactory condition as that shown by the accounts. Our expenditure has diminished, and our income has increased. If any gentleman desires to ask any questions in reference to any of the items, Mr. Webb will, I am sure, be ready to reply to them.

No question being asked,

The SECRETARY said: I beg leave, in the absence of the Honorary Treasurer, to point out that the general result of the

year's finances is this: The receipts from ordinary sources during 1889 amounted to £2,097, and in 1890 to £2,288; showing an increase of £191. The ordinary expenditure in 1889 was £2,061, and in 1890 £2,021; showing a decrease of £39. Thus there is a difference of £230 in favour of 1890 as compared with 1889. I should also mention that all life compositions unfunded up to December last, together with £71 received this year, have just been invested in the names of the Trustees.

The PRESIDENT: I move that this Balance-Sheet be received and adopted.

Mr. PREECE seconded the motion, which was carried *nem. con.*

The following paper was then read:—

## NOTES ON THE DESIGN OF MULTIPOLAR DYNAMOS.

By W. B. ESSON, Member.

Though comparisons have frequently been made of the capabilities of machines having two poles and those having a greater number, I am not aware that the design of multipolar dynamos has yet received systematic treatment in any communication, or that a very satisfactory basis for comparison of the two types has up to the present been suggested. A perusal of the notes scattered about in technical journals, proceedings of Societies, and text-books shows that the ideas on this subject are of the vaguest character, their expression having a most uncertain tone, and generally manifesting some erroneous notion. It is with a view of clearing the ground a little, and inducing some expression of correct ideas from members present, that these notes are brought before the Institution.

### 1. PREVENTION OF SPARKING IN MACHINES GENERALLY.

In the paper which I submitted for your consideration twelve months ago, attention was directed to certain empirical formulæ relating to the armature loads for direct-current machines. From the data furnished by a large number of examples, some of which were given in the paper, an expression for the load which could be safely carried by armatures without causing

sparking was obtained; this expression, though admittedly of an arbitrary character, having been proved by my own experience, and that of my colleagues, to have been of some service. In that paper the term "ampere-turns" was employed to denote the product of the number of conductors on the exterior of the armature and the current carried by each, and I am afraid this may have led to some confusion; at least, so it appears from the discussion. In order that there shall be no misunderstanding in the present case, I shall call this product, quite irrespectively of the number of poles, the *volume* of current carried. The volume is therefore the total current flowing parallel to the armature shaft, independently of direction and whatever the number of poles, and it is obtained by multiplying the conductors on the exterior of the armature by the current flowing in each; or, considering the exterior winding as a copper cylinder, it is the total current flowing in it parallel to the axis.

In his paper on "Armature Reactions," read here last year, Mr. Swinburne had worked out the limiting load from a theoretical point of view, and those who were present will remember that to obtain the results given in his paper the author assumed that the brushes were placed almost close to the polar tips. So far as the practical consideration of the subject goes, it matters little, I think, whether this is strictly accurate or not; because, while moving the brushes back from the tips increases the ampere-turns producing the cross field, the magnetic resistance of the cross circuit is at the same time increased, due to the interposition of an increased air gap. For the moment, however, it will be assumed that the brushes are somewhere near the pole-tips—in their vicinity, let us say—and that the only conductors concerned in producing the cross field are those covered by the pole-pieces. This is sufficiently near the truth for our purpose. Under these circumstances the magnetising force in ampere-turns producing a cross field is, of course,  $\frac{v\phi}{360}$ , where  $v$  is the volume, and  $\phi$  the angle embraced by each pole-piece in degrees. Call  $l$  the length of the air gap, measured from the surface of the armature core to the polar surface, and  $I$  the induction per square centimetre

which would be produced in the gap due to the field magnets alone. When a current flows in the armature the field is weakened at the pole-tips nearer the brushes, and strengthened at those farther from them. Imagine that the armature is loaded so that the forward induction under the nearer pole-tips is just balanced by the cross induction, and we have  $\frac{v\phi}{360} = 2 l \times .8 I$ , assuming that the components of the cross circuit, other than the air gap, have no resistance. According to this equation, with a value  $v = \frac{576 l I}{\phi}$ ,\* the forward and cross induction at the pole-tips would be equal, the field would be *nil* at the brushes, and, the machine being unstable, any increase of current would at once cause great sparking.

The above expression gives what might be called the theoretical load limit, on the assumption that no field is required for reversing the current in the sections as they pass the brushes, but it need scarcely be pointed out that in practice the volume is always much under what would be given by the formula. In the nature of things, one expects in the dynamos of different makers a considerable variation in the relation which the actual load bears to the limit above indicated, and such differences undoubtedly exist; but while one finds, on examining a large number of machines, several under, in few cases are there any having for  $v$  a greater value than half that given by the equation. This large margin must be considered in the light of a safety factor, for it would be folly to build machines the commutators of which would be liable to ruin by an occasional increase of current over the normal. No one expects machines to stand, in continuous working, a current of 50 per cent. over their normal output, for the rise in temperature would then be excessive; but,

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\* I have pointed out to Mr. Swinburne that the equations,  $A = \frac{10 b g B_a}{\theta^2 r}$  and  $A_s = \frac{10 B_a r R l}{w^2}$ , which he gave last year on page 266 of his reply, ought to be  $A = \frac{5 b g B_a}{\theta^2 r}$  and  $A_s = \frac{20 B_a r R l}{w^2}$  respectively. In looking over his paper and reply, Mr. Swinburne finds other slips, which he will probably correct as discussion on the present paper proceeds.—W. B. E.

from the sparking point of view, this increase in a well-designed machine ought to make but little difference to it. Generally, the relation existing between the volume, gap dimensions, and induction in the best machines is expressed approximately by the equation,

$$v = \frac{288 \, l \, I}{\phi} \dots \dots \dots (1)$$

which gives, I consider, a very good rule for use in practical work. According to this, the working volume has about half the value expressing the absolute limit, this relation indicating the margin of safe working dictated by practice and experience. The formula expresses, in fact, an empirical relation which has a certain degree of flexibility. But with such a large margin it will be readily perceived that the strict accuracy of our assumption about the position of the brushes is of trifling importance, as is also our assuming that the whole of the magnetising force of the cross field is spent in the air gap.

It is easy to translate the above expression into the form I previously used for cylinder armatures. Call  $r$  the radial depth of the armature core,  $w$  the width of the pole-piece, and  $I_c$  the induction in the core. The induction,  $I$ , in the gap is  $\frac{2 \, r \, I_c}{w}$ ; and substituting for  $I$  its value, we have,

$$v = \frac{576 \, r \, I_c \, l}{w \, \phi} \dots \dots \dots (2)$$

Taking the induction in the armature core at from 17,000 to 18,000 C.G.S. per square centimetre, and inserting an average value of  $\phi$ , we get for bi-polar Gramme-wound machines the expression,

$$v = \frac{r \, l \, 85,000}{w} \dots \dots \dots (3)$$

which I had used for some time, and which was given in my last year's paper. This is only a rough approximation, for, as Mr. Swinburne has pointed out, to be quite accurate the angle should appear in the denominator as in (1) and (2). The simplest form the expression can take is given in (1). This contains nothing but the length of the air gap, the induction in

it, and the angle of the pole-piece. It will be observed that it takes no notice of the diameter of the armature or of the number of poles. So far as the sparking limit is concerned, it gives a rule which may be employed in designing machines of any size and with any number of poles.

## 2. THE RELATION OF $v$ TO THE DIAMETER OF THE ARMATURE.

But besides taking care that the armature load does not approach the sparking limit, we must provide ample surface for getting rid of the heat generated in the conductors. The first thing to be settled in designing a new machine is what this amount of heat shall be; or, in other words, we must fix the ratio which the energy appearing at the terminals shall bear to the total electrical energy produced. Having settled this, sufficient radiating surface must be allowed to prevent too great a rise in temperature—a point to which due consideration has already been given.

The principal factors which determine the relation of the volume to the diameter of the armature are efficiency and temperature. It will be seen that the equation (1) gives no direct information respecting the diameter for a given volume, and as long as  $I$ ,  $l$ , and  $\phi$  remain unchanged, the tendency to sparking would be the same whatever the diameter. But it is not so with the heat generated or the temperature rise, for assuming  $v$  and  $l$  to be related as shown, the smaller the diameter, the greater would be the temperature. To carry a given volume we must have, consistent with the waste of power permissible, a certain section of copper; and this copper should be disposed so that a cooling surface is provided sufficient to keep the rise in temperature within the specified limit, while the gap, being of sufficient length to prevent sparking, should have only the dimensions necessary for accomodating the conductors and allowing of proper clearance.

Through the kindness of members of the Institution and others, who have liberally supplied me with figures, I have been able to ascertain the nature of the relation between the diameter and volume existing in all the best known machines, from the

smallest to the largest sizes. The figures refer to both cylinder- and drum-wound armatures, and include machines with two, four, six, and eight poles. Though I am not at liberty to publish the data in full, the general results are given. There is not so much agreement between the dynamos of different makers in respect to this relation as might have been expected, and for  $v$  we have all kinds of values, ranging from 200 to 1,000 times the diameter of the armature in centimetres. If full advantage of the length of the air gap were taken, and the thickest possible conductor used in each case, the diameter, to give a uniform temperature for all sizes, would be about proportional to the square root of the volume, but there are several reasons why this proportion should not obtain in practice. With this relation the ratio of the stray to the useful field would increase with the diameter, thus entailing an extravagant expenditure of energy in producing the requisite gap induction. Again, while the total field through the armature would increase simply as the diameter, the volume carried would increase as the *square* of the diameter, this being at variance with the well-established rule that the total field through the armature increases rather than diminishes relatively to the volume as the size is increased. It will be understood, of course, that precisely the same result is arrived at whether we consider the volume fixed and endeavour to find the best diameter, or consider the diameter fixed and seek for the best volume. It is simply a question of obtaining the most economical construction, having regard to cost of materials, efficiency, prevention of sparking, and temperature limit, though the figures at my disposal show estimates of the relative values of these factors to be by no means uniform.

Though the relation lacks definiteness to some extent, I find in the data of a large number of machines indications sufficiently pronounced to justify us in regarding the volume carried by the armatures of two-pole dynamos as proportional to the diameter for all sizes. In designing cylinder machines the value of  $v$  may be taken as 400 times the diameter of the armature in centimetres, while for drum armatures the volume is obtained

by multiplying the diameter by 600.\* The cylinder armature has for a given volume a larger diameter, because of the influence of the interior wires. These being heaped inside to one and a half or twice the depth of the exterior winding, also being longer, a larger diameter is required for a given volume, both from efficiency and temperature considerations. Necessarily, the relations here given are not of a hard-and-fast character, and may be varied considerably. But whatever the proportion adopted, it is absolutely essential that the sparking limit already considered be not too closely approached.

In machines having four and six poles the same average relation between the volume and diameter holds in practice for both cylinders and drums. In the calculations which follow, these figures will therefore be adopted.

### 3. OUTPUT OF DIRECT-CURRENT ARMATURES.

If we call  $N$  the total number of lines of force entering the armature from all the poles, however many, and  $n$  the number of revolutions per second, the average E.M.F. generated in each conductor is, of course,  $N n 10^{-8}$ . If  $C$  be the total current flowing, each conductor will carry with the sections coupled in the ordinary way  $\frac{C}{p}$  amperes,  $p$  being the number of poles. The electrical work due to each conductor is  $\frac{C}{p} \times \frac{N n}{10^8}$ ; and the total work,  $\frac{w C}{p} \times \frac{N n}{10^8}$ , where  $w$  is the number of conductors, counted all round the exterior of the armature. The quantity  $\frac{w C}{p}$  is what we have called the *volume*, and we get for the total electrical output in watts the expression,

$$W = N n v 10^{-8} \quad \dots \quad (4)$$

which is quite independent of the manner of coupling up the armature sections. It will be evident, I think, that with the same relation existing between  $v$  and  $d$  for two, four, and six poles, the output of an armature of given diameter and length,

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\* In my last paper the number given was 570. This I now amend as above — W. B. E.

running at the same speed, is quite independent of the number of poles. It matters not whether  $N$  be furnished by two poles only, or by four poles of half the angular width, provided its value remains unaltered.

The volume, then, may be expressed in terms of the diameter; the quantity  $N$  may be expressed in terms of the diameter  $\times$  length of the armature. Taking an induction of 5,000 C.G.S. units per square centimetre in the air gap—a very usual figure—and assuming that the fraction of the armature circumference covered by the pole-pieces =  $2.25 \, d$ , we get, calling  $L$  the length of the armature in centimetres,  $11,250 \, d \, L$  as the total number of lines of force entering the core. Call this, in round numbers,  $12,000 \, d \, L$ . Substituting for  $N$  and  $v$  their values, the output becomes for cylinder armatures

$$W = .048 \, d^3 \, L \, n \quad \dots \quad (5)$$

and for drum armatures

$$W = .072 \, d^3 \, L \, n \quad \dots \quad (6)$$

This applies to all direct-current machines, whether bi-polar or multipolar; though it will be understood, of course, that the induction in the gap may be greater or less than what has been assumed. In that case the coefficients .048 and .072 would be altered, without changing, however, the form of the expression.

#### 4. OUTPUT OF ALTERNATING-CURRENT ARMATURES.

The armature loads for alternators, though producing a cross magnetisation, as in direct-current machines, raise no considerations, of course, as regards sparking. But on account of the greater proximity of the poles, and the greater stray field resulting, it is desirable to make the layer of copper on the armature core as thin, and the air gaps as short, as possible. As a consequence, probably, of the increased ratio of stray to useful field, the induction is less in the gaps of alternating- than in those of direct-current machines. Here it will be assumed that the virtual induction is 4,000 C.G.S. units per square centimetre, meaning that this represents the induction to which the resultant E.M.F. is due. The impressed E.M.F. is greater than the resultant, and the induction due to the field magnets alone

is greater than 4,000; but what I am dealing with is the field resulting from the inter-action of the magnet and armature fields: it is the field to which the resultant E.M.F. =  $C R$  is due, this and the current being coincident in phase. Usually the poles cover a fraction of the circumference =  $1.5 d$ , the value of  $N$  for alternators being then  $6,000 d L$ . About half the circumference being covered with wire, under usual conditions of temperature and efficiency we have for  $v$  a mean value of  $400 d$ , which gives for the product of mean resultant E.M.F. and mean current =  $.024 d^2 L n$ . In some cases, as in the Westing-house machine, the conductors cover considerably more than half the circumference, and  $v$  is therefore greater. But here, on account of the differential effect produced on the separate wires of the same section, the added turns have nothing like a proportionally increased value, and the expression will give approximately the value of the product of mean resultant E.M.F. and mean current, even in these machines. Assuming that the E.M.F. is a sine function of the time, to get the watts we must multiply by  $\frac{\pi^2}{8}$ , which gives us, therefore, as the output of the alternator,

$$W = .0296 d^2 L n \quad \dots \quad (7)$$

or rather over 40 per cent. of the work done by a drum-wound direct-current machine having an armature of the same external dimensions. Some comparisons have been made of the output of direct and alternating machines of the same weight, but these, in the nature of things, must be misleading. The two types are quite unlike in their proportions, as Figs. 1 and 2 show, and no one would think of making them the same. Fig. 1 represents the magnetic system of a four-pole direct and Fig. 2 that of a 12-pole alternating machine of the same output; and while it will be observed that the iron parts—core and yoke-rings—are much heavier in the former, it will be noticed that the copper in the magnet coils is much heavier in the latter. In fact, though the iron in the alternator is only 55 per cent. of that in the direct-current machine, the copper required is no less than 250

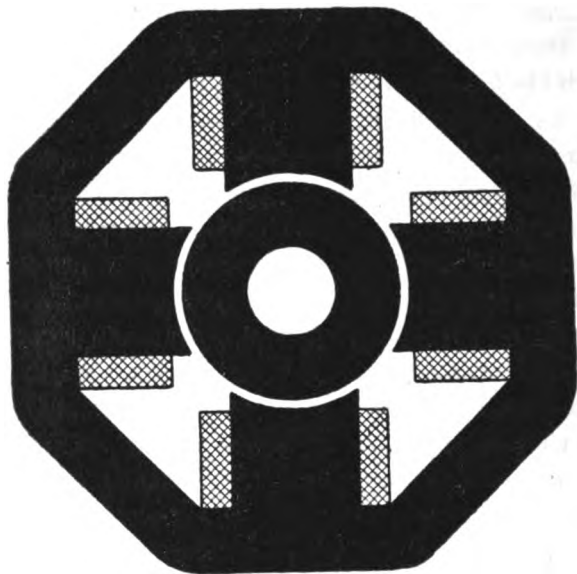


FIG. 1.

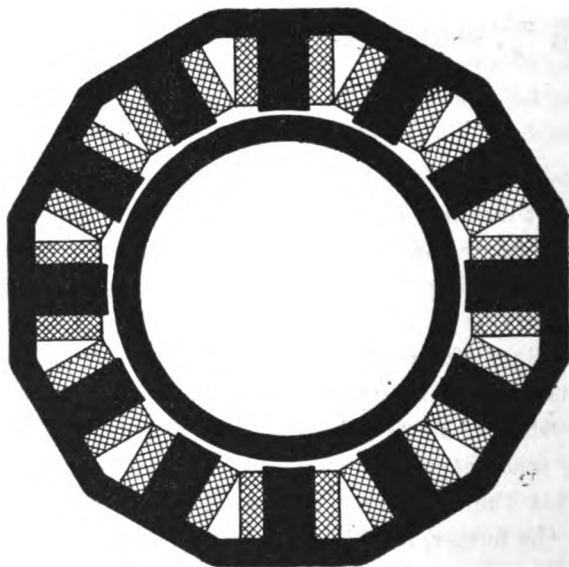


FIG. 2.

per cent. It is difficult, then, to understand what useful purpose is served by comparisons such as I have alluded to.

### 5. RELATION OF AIR GAP TO DIAMETER OF ARMATURE AND NUMBER OF POLES IN DIRECT-CURRENT MACHINES.

Having established the proportionality of the volume and diameter, it is easy to find the relation which must exist between the diameter and length of air gap for any particular angle of embrace, in order that sparking may not occur. I need not take up time ringing changes on the several equations, as to obtain the relation now referred to, all that has to be done is to substitute for  $v$  in (1) its value in terms of the diameter accordingly as the winding is of the cylinder or drum type, and find the connection between  $l$  and  $d$ . Preserving the same safety factor throughout, it will be found that two-pole dynamos with a mean gap induction of 5,000 C.G.S. units per square centimetre, and pole-pieces embracing an angle of  $130^\circ$ , must have—the volume being related to the diameter as described—a gap of not less than  $\cdot 036 d$  if cylinder-wound, or  $\cdot 054 d$  if drum-wound. As will have been observed, the air gap may diminish as the induction is increased or as the volume is reduced.

But, as is also seen from the equation, the gap required for any particular volume is proportional to the angle of embrace, and if we substitute for two poles a greater number, of correspondingly less angular width, working with an increased diameter and volume without a proportionally increased gap is made possible. This is where the advantage of the four-, six-, or even eight-pole machine comes in. Keeping to two poles, increasing the diameter requires either a proportional increase in the gap, whether the space is required for the conductors and clearance or not, or an increased induction, or a diminished polar angle, or a combination of these. In either case, the magnetising force spent in the gap is increased; and, other things remaining the same, obviously it would be of some advantage to adopt a construction which, while producing no greater tendency to sparking, would admit of the air gap being reduced until its length was just sufficient to accommodate the conductors and allow of the necessary clearance. The work done by an armature of given external dimensions we have seen to be quite

independent of the number of poles, and the choice of this number can only be a question, therefore, of structural and working economy.

## 6. DIMENSIONS OF THE ARMATURE.

It has been observed that the output of an armature is proportional to  $d^2 L$ ; and the induction being the same, the weight of the core for a given number of poles must be proportional to the output, the radial depth increasing directly as the diameter, so that a proportionally increased field may be carried. The number of poles being fixed, the weight of the core for a given output may be taken, therefore, as approximately constant, whatever the ratio of  $L$  to  $d$ .

As the number of poles is increased, the induction remaining the same, the radial depth of the core is diminished in proportion, and, within the limits of practice, we may make the further assumption that the weight is inversely as the number of poles. The money value of a reduction in the weight of material due to increasing the poles can easily be arrived at.

The power wasted in hysteresis is proportional to the weight of iron magnetised, and to the number of reversals per second. The weight being inversely, and the reversals directly, as the number of poles, the power wasted is for a given output the same; furthermore, as the output is proportional to the speed, we may say that for a given induction the loss in hysteresis is about proportional to the output only, without reference to speed of rotation, weight of core, or number of poles. If it be more important to reduce the loss by hysteresis than to reduce the weight of material, it can, of course, be done. It is a point for the designer to consider.

So much for the core; let us now consider the copper part. Taking Gramme-wound armatures having an interior opening equal to  $\cdot 66$ , or two-thirds of the core diameter, it is found—the output, speed, and temperature being predetermined—that the ratio of  $L$  to  $d$  may vary through a considerable range without making any great difference in the weight of copper or efficiency.

For instance, the most efficient relation being  $L = \frac{d}{3}$ , it may be

varied on the one hand till  $L = d$ , or on the other till  $L = \frac{d}{6}$ , without increasing the copper by more than about 10 per cent. Of course the watts wasted in the armature are correspondingly increased, but within the limits of the large variation mentioned the reduction in the electrical efficiency of the machine is under one-half per cent. In two-pole machines  $L$  varies from  $\cdot 5$  to  $1\cdot 5 d$ , the normal relation being about  $L = d$ . As has been seen, the gap has to be increased in proportion to the diameter, unless a greater number of poles be employed; and the disadvantage of an increased magnetising force would, in machines with only two poles, counterbalance the slight advantage of getting the armature dimensions nearer the best proportion. When the poles are increased, however, the gap may remain fixed, and if the radial depth of the core be correspondingly diminished, the proportions for least copper and highest efficiency are altered: thus in a four-pole Gramme we can work from  $L = \frac{d}{3}$  up to  $L = \frac{d}{12}$  without a greater variation than 10 per cent. in the weight of copper. Observe, this is a question differing altogether from the one which was considered in my former paper. In that case the length and radial depth of the armature over the winding were fixed, the problem then being to find the best relation of copper to iron. Here, both radial depth and length of core alter, also the peripheral velocity, though the revolutions per minute remain the same. Why the velocity is allowed to alter will be immediately apparent; I only show at the moment that the dimensions of the armature may have their relations altered considerably without making any considerable difference to the weight of copper or efficiency.

To drum-wound armatures precisely the same reasoning applies. Here, speed and temperature being fixed as before, the best proportion for two-pole machines is about  $L = 3\cdot 3 d$ ; but because of the distance between the bearings which such a relation would necessitate being inconveniently great, we rarely find  $L = 2 d$  exceeded. The latter requires about 5 per cent. more copper than the former; while for the usual proportion,

$L = 1.5 d$ ,  $12\frac{1}{2}$  per cent. more is required than for  $L = 3.3 d$ . The variation which can be made without overstepping the limits of economy has a smaller range in drum than in cylinder machines, owing to the greater relative importance of the end wires, because of their greater length. If the length of the core is reduced below  $L = 1.5 d$ , the copper in machines with two poles increases rapidly; but if the number of poles be increased, the length of the end wires being shortened nearly in proportion, the core length may be reduced to a fraction of the diameter without sacrifice of copper or efficiency. In a four-pole machine, for instance, the same copper is used with  $L = .5 d$  as in a two-pole one with  $L = 2 d$ ; while for six poles, without increasing the copper, the relation may be as small as  $L = .25 d$ . The effect of adding poles when the diameter is relatively great is not so noticeable in Gramme-wound machines, as the end wires are less important.

All the above facts go to prove that if there is anything to be gained, as regards the production of the field, by increasing the diameter of the armature and the number of poles, there is nothing, considering the armature by itself, to be lost by it. It may, in fact, be rather an advantage, because the weight of the iron core is reduced. It is true the peripheral velocity is increased; but this does not matter in the least, provided a certain limit is not exceeded. Opinions differ as to what the limit should be, some machines working at 50 feet per second, others at 100, and a few as high as 125. But there is no reason whatever why any properly constructed armature should not run at a periphery speed of 100 feet per second; and provided this velocity is not exceeded, any advantage which may be obtained by a relatively large armature and increased number of poles should be secured.

## 7. DIMENSIONS OF THE FIELD MAGNETS.

It will be apparent, from the foregoing considerations, that the employment of two, or more than two poles for direct-current machines of moderate dimensions resolves itself mainly into a deliberation regarding the most economical shape to give to the

field magnets. As regards the armature, considered by itself, we may say that the choice of dimensions is mostly a matter of convenience, seeing that the amount of copper required and efficiency are for a given output practically unaltered by variations in this respect, while the reduction in the weight of the core due to an increased number of poles is to some extent compensated by the extra expense of larger plates and increased weight of the armature supports. Again, the cost of labour is increased by the larger diameter; but, everything being taken into account, considerations respecting the armature do not influence the design to a very great extent. One thing in favour of increasing the poles, as far as the armature is concerned, must, however, be remembered, and that is the reduction, consequent on a smaller conductor being used, of the losses arising from parasitic currents. We now turn our attention to the magnets.

I have said that for the prevention of sparking it is necessary that, the induction per square centimetre remaining the same, the air gap increases proportionally to the diameter, whether the space is necessary for conductors and clearance or not, but that the coefficient by which the diameter has to be multiplied to give the length of the gap necessary to prevent sparking diminishes directly as the pole angle. In comparing the magnetic system of a four- or six-pole machine with that of a two-pole one, it is necessary to adopt dimensions for the armature in accordance with the considerations already mentioned; hence, if the two-pole armature had a length of core equal to one and a half times its diameter, in a four-pole one the length should be about half the diameter. The diagrams (Figs. 3 and 4) show the cross sections of two such machines. The diameter of the four-pole armature is 1.4, and its length .5 times that of the two-pole one, consequently both machines give the same output. The weight of the two horse-shoe magnets in the four-pole machine comes to 56 per cent. of the weight of the single horse-shoe magnet, which indicates in this particular case a considerable saving in wrought iron. In taking the copper weight it is necessary to bear in mind that this does not vary simply as the length of the wire if

the machines are of the same efficiency, but as the square of the length; so in this particular comparison the copper on the two

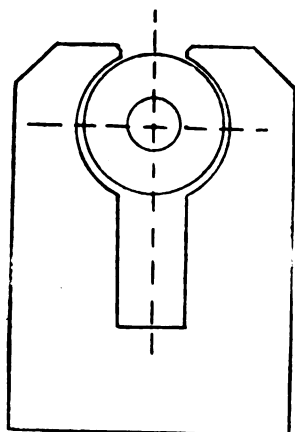


FIG. 3.

horse-shoes would be, roughly, 30 per cent. more than on the single horse-shoe, assuming the length of the air gaps to be the same. Now, if it were possible to reduce the gaps by 12 per cent. or so, the copper weight would be similar in both machines, and we should have credit for a certain amount of iron saved in the construction of the four-pole one, which could be balanced against the increased expenditure for labour. If the gap can be reduced by more than 12 per cent.—always retaining the depth of the winding

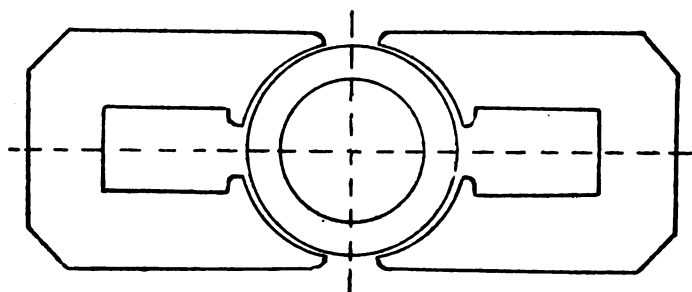


FIG. 4.

the same—there is a saving of copper as well as iron, and it is simply the comparison between the value of the copper and other materials used and the cost of the labour in the two cases which determines, at least in machines of moderate size, whether two or more poles should be adopted.

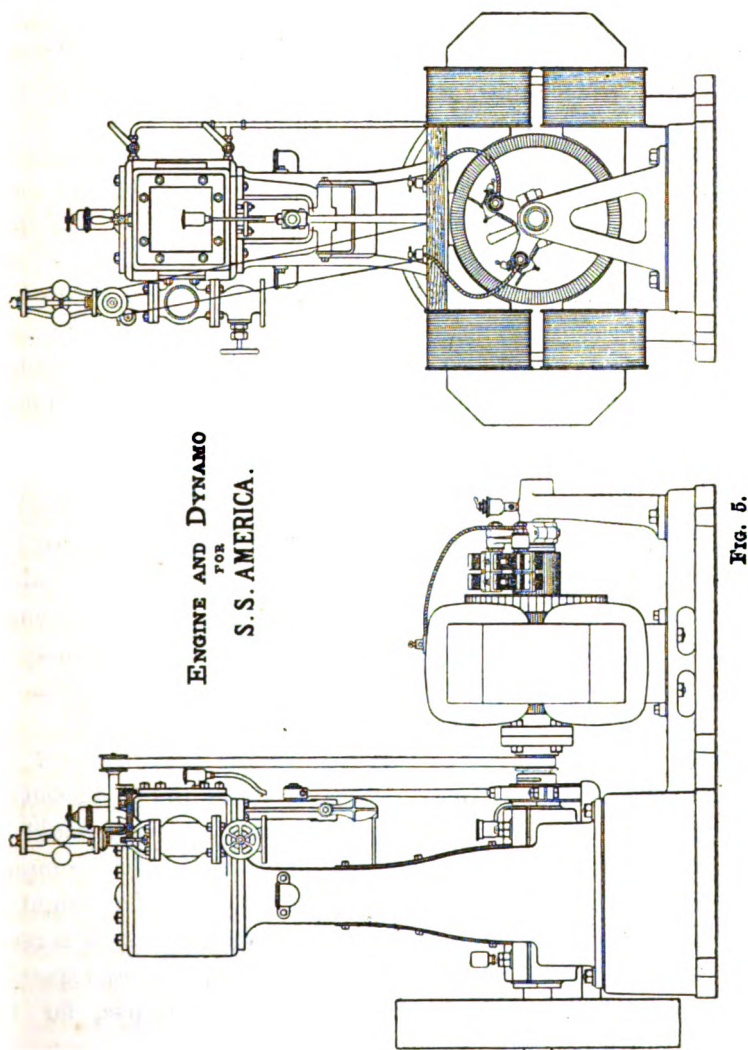
In getting out the best relation of  $L$  to  $d$  in the different types of armatures, it is assumed, of course, that, the volume being proportional to the diameter, the depth of the winding remains unaltered, as that is the condition which gives uniform rise in temperature. Accordingly, for a given output, the layer of copper

on the armature will be of the same depth whether the machine has two poles and an armature having a length of one and a half times its diameter, or four poles and an armature having a length of only half the diameter. But whether it is possible to reduce the gap to an extent which, with an increased number of poles, will lead to a less costly construction, is a question which for machines of moderate dimensions must receive careful consideration in each individual case. The answer depends upon how much larger the gap has to be to prevent sparking in the case of two poles only, than is requisite to accommodate the conductors and allow the necessary clearance. If the difference is considerable, it may pay better to add poles, and reduce the gaps that way, than to do the same thing by diminishing the pole angle or increasing the induction, or both. The question, it appears, is answered in different ways by different people. It is somewhat interesting to note, for instance, that one engineer, distinguished for the past six years as an ardent advocate of multipolar machines, has, after reducing from six poles to four, lately arrived at two, while another has jumped straight away from two to six without a halt at the intermediate number.

But when we come to machines of a certain size there is undoubtedly a gain in employing more than two poles, while very large machines become impossible of construction with two poles only. Consider the case of a two-pole armature of 60 centimetres diameter. Assuming the induction in the gap to be just over 5,000 C.G.S. lines per square centimetre, and the pole angle  $130^\circ$ , our calculations show that the length of air gap would require to be 3.2 centimetres. This is some 40 per cent. in excess of the requirements of conductors and clearance; so, if the gap is reduced until it just allows of the requisite clearance, the induction must be increased from 5,000 to 7,000. Keeping the output of the machine the same, however, we might reduce the diameter of the armature to 54 centimetres, and work with an induction in the gap of 6,500. Observe, the magnetising force spent in the gap has now been reduced by about 7 per cent., but the total field through the armature has been increased by 15 per cent., and the induction in the armature per square centimetre by more than

25 per cent. If we retain the same section of iron in the fields, it may be assumed then that about the same total magnetising force is required, whether the machine has an armature 60 centimetres in diameter, with a gap induction of about 5,000 C.G.S. units, or one 54 centimetres in diameter, with a gap induction of 6,500. The latter might turn out to be impossible owing to the greater heating of the armature core ; any way, what I am attempting to show now is, that by pressing up the induction with a view of reducing the gap, little, if any, advantage is obtained, as what we gain in one direction is lost in another. But by increasing the number of poles, and reducing the gap in that way, we effect, without increasing the gap induction, a marked economy. As it is unnecessary to go into a mass of figures to prove what each can readily prove for himself from the data already before you, I will just give here the results. If we substitute for the two-pole armature of 60 or 54 centimetres diameter and 90 centimetres long a four-pole one of 84 centimetres diameter and 45 centimetres long, with a gap induction of 5,000, we require for the magnetising coils while working at the same sparking limit, and with the same efficiency, rather under 70 per cent. of the copper on the two-pole field. When to the saving in copper there is added the saving in iron, there will be, after the extra labour is debited against the four-pole machine, a considerable balance in its favour. But to go beyond four poles in this case would be a mistake. Increasing the poles, though it reduces the iron, always results in an increase of copper, unless at the same time the power spent in the gap is reduced, and this latter must be effected without reducing the thickness of the armature winding. The economy shown above is simply due to the fact that in a two-pole machine of the dimensions specified the gap necessary for the prevention of sparking must be much larger than the conductors require unless pressed up to a high induction. If with four poles the gap is still larger than necessary, we go to six poles, and so on. But when we arrive at a point where increasing the number admits of no reduction in the gap, there is no need to go farther. It may be mentioned, however, that in sizes where a four-pole construction showed no actual economy in first cost, it might still be

preferable to the two-pole on account of its symmetrical field and the absence of magnetic pull. The advantage of the light-weight multipolar field for slow-speed railway motors will, of course, be obvious.



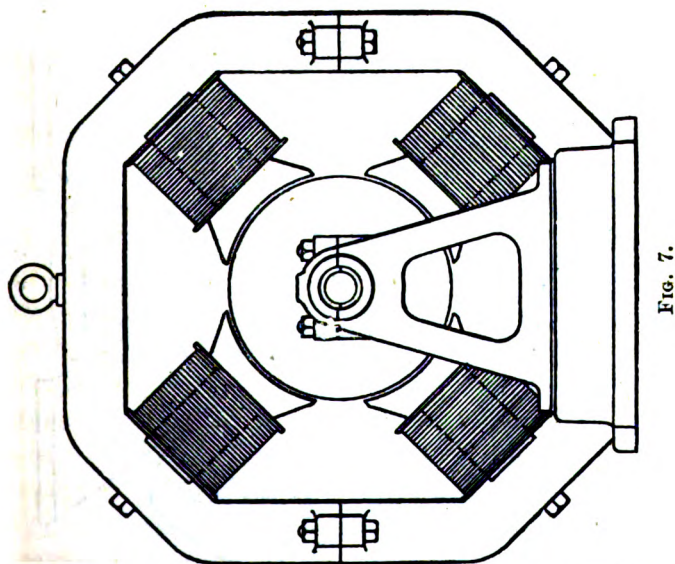
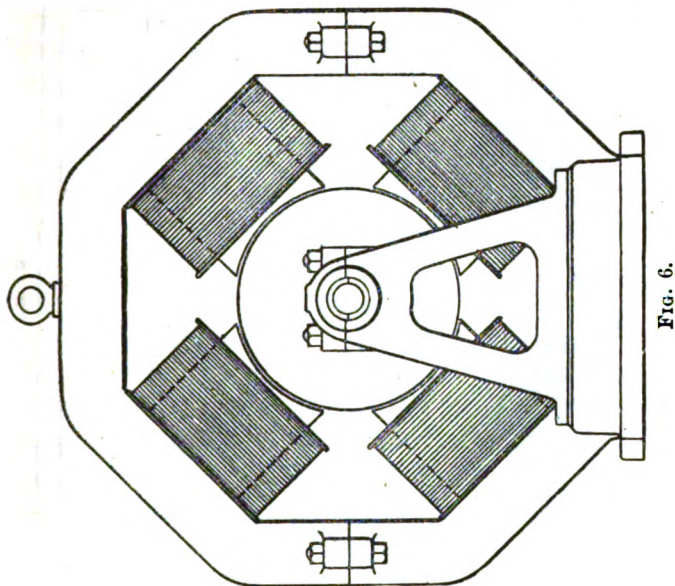
Having shortly discussed the design of multipolar machines as influenced by both theory and practice, it remains to conclude this communication by calling attention to some of the ordinary

forms of multipolar fields. The double wrought-iron horse-shoe (Fig. 4) is not very frequently used, being rather costly; but a similar machine with magnets of cast iron, lately made by my firm for the National Line s.s. "America," is shown in Fig. 5. The armature core in this case was formed by winding square annealed-iron wires on a gun-metal flanged cylinder. The winding was of the Gramme type, and the machine was coupled direct to an inverted engine, as shown in the figure. This design, introduced by Gramme in 1869, frequently gives place to the arrangement shown in Fig. 6, a form designed by the same inventor in 1885, and since adopted for cylinder-wound armatures by many makers, including Mr. Jaspar, in Belgium; Mr. Brown, of the Cernikow Works, in Switzerland; and Messrs. Paterson & Cooper, in England. Lately Mr. Kapp has used the same form for six- and eight-pole machines with drum armatures. The magnets and octagonal yoke in Fig. 6 are of cast iron, in two pieces, the lower limbs, the bottom half of the yoke-ring, and bed-plate being one casting, and the top limbs and upper part of the yoke being another. Fig. 7 represents a similar field in which the magnet cores are of wrought iron, fitted with cast-iron pole-pieces, as used by Mr. Kapp in the machines above mentioned. The decision as to whether cast iron or wrought iron should be used, is arrived at in a very simple manner—by comparing the excess of copper required on one hand with the extra machining required on the other.

It will be observed that in the designs Figs. 6 and 7 the yokes are considerably longer than those shown in Fig. 4; and the weight of the former, if made of the same material, would be, roughly, twice that of the latter, though even then the complete magnet system would be but 75 per cent. of the weight of Fig. 3. The yokes being of cast iron, however, the system really comes about 20 per cent. heavier than Fig. 3; the less expensive character of the material compensating, of course, for the increased weight.

All these are examples of single magnetic circuits, where the lines of force from each pole remain undivided in their paths through the magnetising coils; but in Figs. 8 and 9 are shown

examples of double magnetic circuits, in which the lines from each pole take two paths through separate coils. Fig. 8 is a type



of magnet used by Sautter-Lemmonier, of Paris, for Gramme-wound armatures, and by Cuenod-Sautter, of Geneva, for arma-

tures having a Siemens winding as modified by Thury. The magnetising coils are wound upon the parts of the system

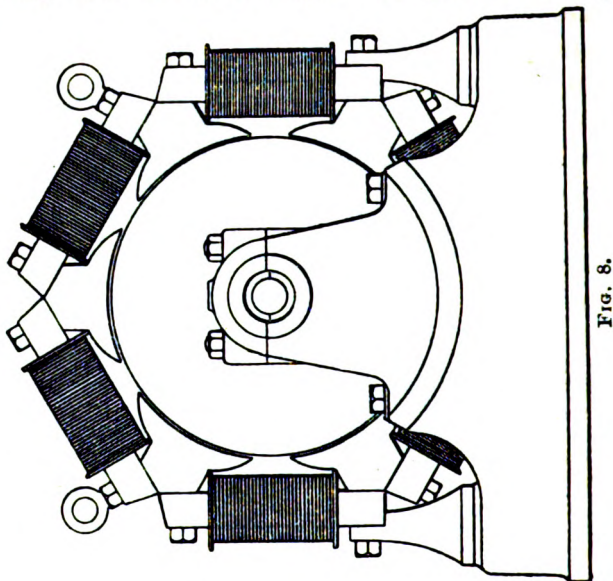


FIG. 8.

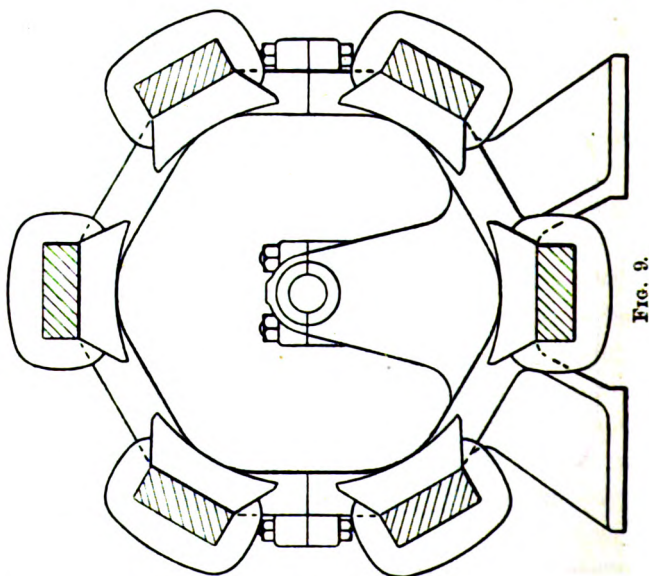


FIG. 9.

constituting in Fig. 7 the yokes, and a greater amount of copper is in consequence required. It looks at first sight as if the weight

of copper were not very different in the two types, but in this respect appearances are deceptive, for, as a matter of fact, the field of a four-pole machine made to Fig. 8 would require about 125 per cent. more copper than if made according to Fig. 7. It will be observed, however, that the magnet cores and pole-pieces, which are made throughout of the softest wrought iron, are very light. In Fig. 9 the magnets are a series of wrought-iron bars lying parallel to the armature, each fitted with a cast pole-piece in the middle of its length, and having two magnetising coils, one on each side of this piece. It is a structure which may be frequently met with, though not always in the form shown, and the observations regarding the copper, made with reference to Fig. 8, apply equally here.

The cost of a four-pole machine is approximately represented by the cost of a couple of two-pole machines of the same efficiency which give each half the output at twice the speed; the cost of a six-pole by that of three two-pole machines which give one-third of the output at three times the speed, and so on. For comparison, the fields must be in both the multipolar machines and two-pole machines of the same character—that is, with single or double magnetic circuits.

The fields of alternators may be similarly divided into those having single and those having double magnetic circuits; the former requiring, as in the fields of direct-current machines, much less copper than the latter. Amongst those having single magnetic circuits are the machines of Siemens, Ferranti, Mordey, Westinghouse, Elwell-Parker, and Paterson & Cooper. Mr. Kapp possesses the distinction of having the only alternator with double magnetic circuits. The "Phoenix" alternator, shown in Fig. 10, possesses some features which may be of interest as illustrating the way in which the commercial aspect of designing has to be considered. The yoke-ring is of cast iron, but the magnets are of tooled wrought iron, shaped as shown. If the magnets had a breadth equal all the way up to the length of the armature core, they might as well have been of cast iron, for the little advantage consequent on the reduction of copper obtained by reducing the width, would not have paid for the extra work in

tooling wrought iron. But when we reduce the breadth where the magnetising coils are, as shown, we at once diminish the copper on the fields by 60 per cent., greatly reduce the leakage

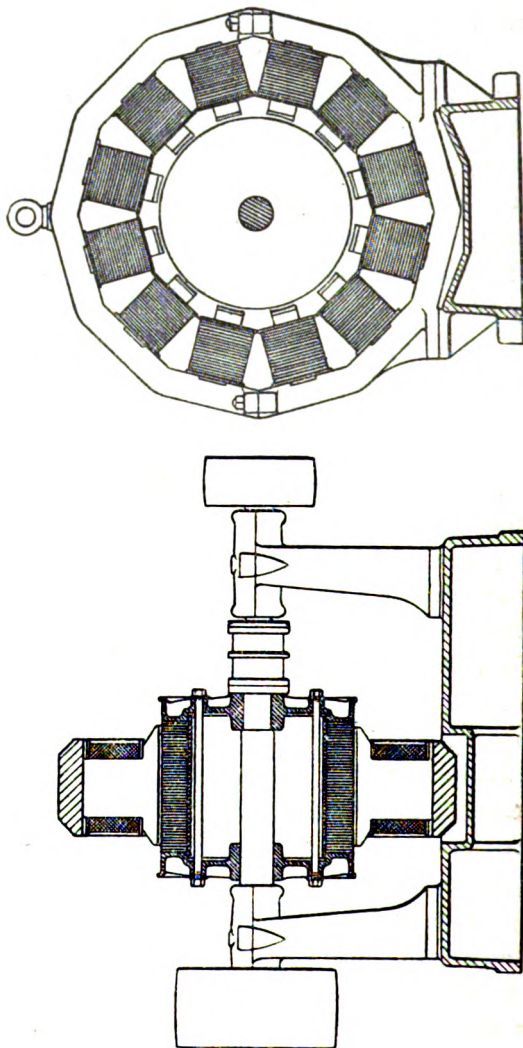


FIG. 10.

area, and get a good balance after paying for extra tooling. The machine, as will be seen, has 12 radial magnets, and there are on the armature six flat coils, each equal to three times the width of

the magnet cores, and laid on the periphery with a space equal to the core between them.

The length of the paper prohibits me from dealing with many special types of machine, to which, however, all the reasoning here used may be applied without difficulty.

The PRESIDENT: Before the discussion commences, I propose a vote of thanks to Mr. Esson for his very able paper.

The resolution was carried unanimously.

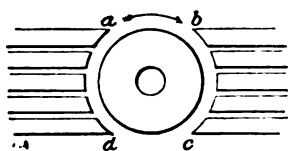
Professor SILVANUS THOMPSON: I am sorry Mr. Esson has used the word "volume" for something that is not a volume at all, and also that the quantity he has called by that term—volume—has been taken of the magnitude as it is. He takes a value not equal to the ampere-turns on the armature regarded as an electro-magnet, but twice as much. I cannot conceive what advantage there is in taking this number as twice as much as the ampere-turns that magnetise the armature.

Professor  
Thompson.

There is a formula given for the theoretical load limit about which I would ask whether it is not incorrect in this respect—that it allows nothing for leakage of magnetic lines. It assumes that all the magnetic lines come from the pole-pieces and pass into the armature without any leakage. Another point that I wish to put to Mr. Esson is this. He asks us to compare certain forms of two-pole machines with certain forms of four-pole machines—notably where he draws two figures, 3 and 4, and presents them for comparison. Now it seems to me that it would have been a more happy comparison if he had contrasted Fig. 4—a four-pole machine—with a two-pole machine not of the shape of Fig. 3, but one which had a double magnetic circuit, half on the right and half on the left, for then we should have had exactly the same amount of surface on the armature from which to lose heat. I do not think there would have been any difference whatever in the output of those two machines. You would have the same heating surface, the same gap, the same total polar surface, the same everything, except that one has two poles and the other four. There is another point upon which I think we might have sought for a little more accuracy of definition. Mr. Esson gives

Professor  
Thompson

us some figures, which we are certainly glad to have, as to a kind of average proportion useful in designing between the value of what he calls the "volume" and the diameter of the armature—400 times the diameter in ring machines, and 600 times the diameter in drum machines. Now, would it not have been well to distinguish between ring machines which have single magnetic circuits and those which have double magnetic circuits? I should like to ask the makers of ring machines with double magnetic circuits—Mr. Crompton, for instance—whether they do not find it expedient to have different proportions in the two cases. Certainly it is well ascertained that the ring machines which have double magnetic circuits are capable, without getting to the sparking point, of being loaded with a greater load of current-circulation on the armature than those which have a single magnetic circuit. The distortion of the magnetic field under the horns of the pole-pieces is less with the double magnetic circuit machines than in those with the single magnetic circuit.



It occurs to me to mention here a suggestion I made years ago to one of the manufacturing companies to prevent the effect of the armature circulation in distorting the magnetism of the poles. The arrangement I suggested—I do not know whether it was ever carried out—was this. Taking a familiar type of machine, the tendency of the circulation of current in the armature is to strengthen the magnetic field under the two tips, *a* and *c*, and to weaken it under *b* and *d*; and as the load on the armature becomes very great the field at *b* and *d* may be almost neutralised. My suggestion was that the field magnets, instead of being of one solid piece, should be made up of a number of parts—laminated, if you like to call it so, but not in the ordinary way, nor for the ordinary purpose. Divisions should be made longitudinally, as indicated in my sketch, so as to prevent the distortion of the magnetism. The introduction thus of magnetic resistance longitudinally will obviously do something to ameliorate the distortion, but will not effectually prevent it.

Mr. Esson gives us a multipolar direct-current machine in Fig. 1, to be contrasted with Fig. 2, a multipolar alternating-current machine; and he points out that the ratio of copper to iron is very much greater in the second machine than in the first, and says, therefore, you ought not to compare the output of a machine wound to give a direct current with the same machine wound to give an alternating current, because it would not be expedient to follow the same design of iron-work for the two. Looking at Fig. 2, it strikes me that the proportion of copper to iron is something prodigious. The section of it seems to me to indicate that the cross section of copper, as compared with iron, in the field-magnet part is as one to three, which is an enormous quantity of copper to put on the iron. It occurred to me to ask Mr. Mordey, who was sitting next to me, what is the ratio of copper to iron in his alternating-current machine. He tells me it is as 1 to 16. But why should Mr. Esson take an alternating-current machine of an obsolete type to compare with the most modern form of multipolar direct-current machine? It would have been better to take a more modern form of alternating-current machine.

Professor  
Thompson.

In conclusion, allow me to say I thank Mr. Esson very much for the many good practical things he has put in this paper,—for the very convenient way in which he has tabulated for future use a number of useful data about proportions of machines. The data given here, though previously known to a few, will be extremely useful to have in this handy form, and will be decidedly of advantage in designing future machines.

Mr. G. KAPP: At the Inventions Exhibition in London there was a machine exhibited by Mr. Joel, and it actually had these notches which Professor Thompson has shown on the blackboard; but they were very shallow. The object which I believe Mr. Joel had was to get a strong magnetic pull. In those days the idea was prevalent that to get a strong magnetic pull you wanted to make a lot of corners. This idea was wrong, but the design resulting from it was a step in the right direction, only Mr. Joel did not go far enough. As a matter of fact, where possible, I design magnets with one deep slot in the middle, in order to

Mr Kapp.

Mr. Kapp.

reduce the disturbing influence of the armature current. Mr. Esson gives, in Fig. 4, an illustration of a four-pole machine, which, I believe, has been first used by Elphinstone & Vincent, and states that it is somewhat lighter than the symmetrical four-pole field first used by Gramme. I should like him to tell us, in his reply, whether in making this calculation he has taken into account that in one case he deals with wrought-iron, and in the other with cast-iron, magnets. Taking the same material, I find that the Elphinstone-Vincent field is a little heavier than the symmetrical Gramme field. Again, the four-pole iron-clad field with only two magnet coils is heavier than the Elphinstone-Vincent field.

In dealing with the question of heating, Mr. Esson has said nothing about the great cooling effect he gets with multipolar armatures. In the two-pole armature you naturally squeeze everything into as small a space as possible. In the multipolar armature you are obliged to get a large diameter, because you want a certain distance between the edges of the pole-pieces. This is almost constant, whether the machine is bi-polar or multipolar; but the fact of having to provide this empty space forces you to make an armature of large diameter; and since you split the field into four or six poles you have a less number of lines coming out of each pole, and consequently you can use a shallow armature core. Thus you get these two effects: first, you get a larger diameter, and therefore a larger radiating surface outside; and, secondly, a shallower core, and therefore a considerable radiating surface inside. Moreover, in a multipolar machine you have greater linear speed than in a two-pole machine; and Mr. Esson, in comparing the heating of these machines, has overlooked this fact. Mr. Esson's formula for current-volume appears to be a simple and practical rule, and in testing it on different machines I find it agrees very well with experience. But it is only applicable to machines of the usual proportions. If, for instance, you take a two-pole machine and double the diameter of the armature and the number of conductors, but do not increase the total field—that is to say, leave the magnets of the same section—you would find that the

formula gives you far too large a current-volume. I draw Mr. Kapp. attention to this to avoid the indiscriminate use of Mr. Esson's otherwise excellent formula in cases where it is not properly applicable.

Mr. ANDERSEN : I should have liked to have more time to study Mr. Esson's paper before making my remarks. It contains a great many things that I have not been used to express in the same way as Mr. Esson. There is only one point which I have taken notice of, and on which I am prepared to speak now—that is, the formula for the load to be carried by the armature. I should like to know whether Mr. Esson does not think it necessary to introduce into this formula any expression for the number of segments in the commutator. I do not think that if we have, by using Mr. Esson's formula, determined a suitable load for the armature, then the same result in regard to sparking would come about if the number of commutator segments was varied. Take a big machine for a large current—say 1,000 amperes—which, as a rule, only has one turn of conductor per segment: it is not seen from the formula what the result would be if we gave the machine such a commutator that it would have two turns in each section. I think that would alter the condition as to sparking seriously.

The discussion was then adjourned.

A ballot for new members took place, at which the following candidates were elected:—

*Foreign Members:*

Holbrook Cushman.		John Van Vleck.
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*Associates:*

Hugh Henry John William Drummond.		Frederick William Schiller.
Herbert H. Fawckner.		Robert Shepherd.
John D. Scott.		R. P. Wilson.

*Students :*

George A. Bruce.	Arthur Edward Langdon.
Frederick Hardress Chaplin.	William Michael Rogerson.
Edward Cuthbert Sylvester	Sydney Cummins Smith.
Findlay.	Ernest John Welfare.
Lionel Frederick Gosling.	Norman Whichello.

The meeting then adjourned.



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...	476	10	0

Telegraph Juries run—

£150 18 6 India 3½% Stock, cost	...	163 8 0
General Investment Fund—		
£450 0 0 Canada 4% Red. Stock, cost	...	475 10 0
Salomons Scholarship Fund—		
£1,000 0 0 New South Wales 3½% Stock, cost	...	1,037 10 6

Furniture—

As per last Balance-Sheet	...	£168 17 0
Less depreciation, say	...	16 17 0

152 0 0

200 0 0

748 9 5

Overdue Subscriptions estimated to realise

Stock in hand of Institution's Journals and Ronalds'

Catalogue, estimated value

Books, Pictures, &c. (other than the Ronalds' Library),

As per last Balance-Sheet

Add value of Books, &c., since

purchased and presented

Amount due for Advertisements in the Journal, 1890

Cash at Bankers, as shown above

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£6,218 5 1

We certify that we have examined the Books, Vouchers, and Securities of the Institution, and that the above Statement of Receipts and Expenditure and Estimate of Assets and Liabilities are correct, and exhibit the true financial state and condition of the Institution.—

WAGSTAFF BLUNDELL, BIGGS, & CO., CHARTERED ACCOUNTANTS,  
12, Delahay Street, S.W.

March 25, 1891.

FRED. CHAS. DANVERS, } Auditors.  
AUGUSTUS STROH, }



The Two Hundred and Twenty-first Ordinary General Meeting of the Institution was held at the Institution of Civil Engineers, 25, Great George Street, Westminster, on Thursday evening, April 16th, 1891—Mr. W. CROOKES, F.R.S., President, in the Chair.

The minutes of the Ordinary General Meeting held on April 9th, 1891, were read and approved.

The names of new candidates for election into the Institution were announced and ordered to be suspended.

The following transfer was announced as having been approved by the Council:—

From the class of Associates to that of Members—

Alexander Pelham Trotter, B.A.

The discussion on Mr. Esson's paper on multipolar dynamos was then resumed.

Mr. F. V. ANDERSEN: I said, in my remarks at our last <sup>Mr. Anderson.</sup> meeting, that, according to my experience, a formula for the proper load on an armature should take into account the subdivision of the armature conductor, for the same load in ampere-turns will give different conditions as to sparking according to how far we carry the subdivision of the winding. It is the self-induction in each section as its segment leaves the brush that causes the sparking, and the greater the product of the length of a section into the current it carries is, the greater is the tendency for the machine to spark.

I wish to add that the question of sparklessness should be taken in connection with the question of the amount of lead necessary for the brushes.

If we use Mr. Esson's formula, with the constant which he gives we may obtain a machine which will work sparkless, when the brushes are set to the correct lead; but the amount of lead

Mr.  
Andersen.

will vary with certain conditions which are not shown in the formula, and whenever the lead is so great that the commutation takes place "in the vicinity of the pole-tips," as Mr. Esson puts it, then a variation of about 10 per cent. in the current is sufficient to cause sparking until the lead has been altered. Now this should be avoided. Central station engineers want machines on which you can set an average lead and leave the machine to work at the ordinary varying loads without sparking, and this can be attained if the machine is so designed as to have a small lead. The main condition for a small lead I find to be the employment of a field with long, powerful limbs (in addition to a thorough subdivision of the armature conductor). The lead should be kept down to one-third of the angle subtended by the horns of the pole-pieces.

Now the principal gain in cost in making multipolar machines is in the saving derived from making short field magnets; but there is no difficulty in making two-polar machines with short field magnets, and to make them cheaper than multipolar machines for similar performances, if we will put up with the consequence—a large lead. If, however, we are tied down to a given lead on the brushes, the advantage of short field magnets is only apparent, as the load on the armature must be set down with the length of the magnets. I believe, therefore, that if the same standard is to be attained by the multipolar as by the two-polar machines, the latter, being by far the simplest to build, will win in the competition. Theoretically there is no gain in the direction of more output in splitting the material up into many poles.

Mr.  
Housman.

Mr. R. H. HOUSMAN: I think that Mr. Esson is much to be congratulated on the introduction of such a complete and comprehensive term as "volume of current," expressing the product of current by the number of conductors; but I think it should be remembered that that expression "volume" expresses double the total effective magnetising current of the armature. This has been allowed for in taking the cross-magnetising force, or  $\frac{v \phi}{360}$ , which, if analysed, represents the same thing, but it is not

definitely stated, and I think it should be noticed, otherwise it is liable to lead to a certain amount of confusion. Mr.  
Housman.

The whole of Mr. Esson's argument seems to depend upon the supposition that the air space required to prevent sparking is in general greater than that required to contain sufficient conductors to prevent heating. But when he investigates the relation of volume of current to diameter, the argument does not seem quite clear. He states, I think, that for heating alone, supposing everything to be increased in the same proportion—diameter, air space, &c.—and the air space be filled up in the same proportion with copper, the volume would be as the square of the diameter. Now I think it would only be as  $1\frac{1}{2}$  power. If the depth of conductor was the same in all cases, then the rise of temperature would be the same with the volume proportional to the diameter. Now, if the depth of the conductor be increased in proportion to the diameter, the section of the conductor will be increased in the same proportion, but the increase of current that can be carried for the same rise of temperature will only be as the square root of the increase of section. That is to say, the current-density must be diminished so that the total volume will be as the  $1\frac{1}{2}$  power of the dimensions. Then he states that by applying the formula of Mr. Swinburne the volume comes out proportional to the diameter in order to prevent sparking. This being so, the total output, or volume, multiplied by the E.M.F. in a single conductor, comes out proportional to the square of the dimensions—I think Mr. Esson states as the cube. It comes to the cube if the number of revolutions is constant; but if the circumferential velocity be constant, it will come only as the square. That is the limit he attains as given by sparking.

Now, for the limit given by heating, since current-volume, as determined by the heating, will be as the  $1\frac{1}{2}$  power of the dimensions, it follows that the output will be as the  $2\frac{1}{2}$  power of the dimensions, supposing the intensity of the field and the circumferential velocity be the same in all cases. This is the output as determined by heating in the conductor alone. Now there is still the heating in the core to be considered. The loss by hysteresis for the same intensity of induction is proportional to the number

Mr.  
Housman.

of cubic centimetres of iron multiplied by the number of cycles per second. So that, if the number of cycles per second be inversely proportional to the dimensions, the heat due to hysteresis will be proportional to the square of the dimensions; so that the rise of temperature due to heating by hysteresis will be the same for all sizes, supposing the intensity and circumferential velocity be the same. The heating by Foucault currents seems to be proportional to the cube of the dimensions—*i.e.*, simply proportional to the volume of iron. Now, as these Foucault current losses tend to limit the output sooner than hysteresis or armature resistance, it becomes important to consider them separately.

I have described a graphic method of separating losses which I have used for a couple of years or so; it has been quite successful, and I have found it very useful. I find that in general the loss by Foucault currents is considerably greater than the loss by hysteresis, and, working out the results obtained, it seems that for a given rise of temperature the maximum output for a given armature will be attained when the losses due to heating by hysteresis and to Foucault currents are equal to the loss in the conductor. I cannot say whether that is mathematically correct, but it comes so in practice. It is not a very nice point; *i.e.*, considerable variation from that equality does not produce any large variation in output. But still the output is a maximum at that point. It therefore follows that the output cannot be increased quite so fast as  $2\frac{1}{2}$  power of the dimensions, owing to the increase in Foucault currents in larger sizes. Thus the sparking limits output in proportion to the square of the dimensions, and the heating limits it somewhere between the square and the  $2\frac{1}{2}$  power.

Now, with regard to the sparking limit, Mr. Esson seems to have accepted Mr. Swinburne's theory that at the point of commutation the coil which is undergoing commutation is cutting a very small field. This he seems to base upon the assumption that the coefficient of self-induction of the coil undergoing commutation is very small. That he explains by saying that there is practically a closed secondary in the form of a solid iron pole-piece, and the effect of that is to limit the self-induction to those lines of force which

only go through air. Now that is, I think, hardly a justifiable <sup>Mr. Housman.</sup> assumption. The current in the commutating coils is not an alternating current. The change is always in the same direction, the other half of the period being represented by the rotation of the coil through  $180^\circ$ . This is quite a different kind of thing from having an alternating current in the coil undergoing commutation. I think that the coefficient of self-induction which should be taken is that which would be measured by a ballistic galvanometer. Supposing you make an experiment with the armature stationary and the field excited to full strength, then pass a current through the coil whose self-induction is to be measured, and observe the kick of a ballistic galvanometer connected to a coil as nearly as possible coincident with it. Then that will give the effective self-induction of the armature coil. Plotting these values for different currents, it would be found that the curve is not a straight line; in fact, it pretty closely resembles a short section of the saturation curve of the dynamo. This is accounted for by the large proportion of the lines of force which pass through the magnets. Now, taking these values for the self-induction, you may calculate the strength of field required for the reversal of any current, and then, by measuring the strength of the field round the armature, find the point at which reversal can take place without sparking. I have tried this in several cases. With dynamos having slotted armatures and small air spaces the intensity of the field required for commutation at full load comes out somewhere about 3,000; out of a maximum field under the pole-piece, about 7,000. The angle of lead may be calculated pretty fairly from these figures—quite sufficiently closely to support the theory. Now, as a further proof that a strong field is required for commutation, on running a dynamo at about half-load, first with brushes covering a full section, next with the brushes only covering half a section, it will be observed that the angle of lead required in the second case is much greater than in the first case—it is almost as great as it would be with full current. Now, if Mr. Swinburne's theory that a very small field is required were correct, the doubling of the strength of field necessitated

Mr.  
Housman.

by halving the time of commutation would be attained by a very small increase of lead.

The difficulty of variable lead may to a great extent be got over by having special commutating pole-pieces—constant-lead magnets, as I am in the habit of calling them. They are something after the style of what Mr. Swinburne calls reversing pole-pieces, only those that he shows are quite inadequate, the number of turns and section of iron that he gives as necessary being very much too small. Of course this follows on his supposition that a very small field is required for commutation. Now in practice these commutating magnets are substantial cores of iron connected closely with the yoke of the magnet, in effect joining the yoke to the neutral line of the armature, so that the pole-piece covers about a couple of sections of armature at the point of commutation. The number of series turns is found in practice for double-magnet two-pole dynamos to be about 1.25 the effective turns on the armature. That is to say, supposing a drum armature has 40 sections, and one turn to each section, the number of turns will be about 25 for the double-magnet dynamo; whilst if a single magnet be used, the number of turns is about .75 the effective number of turns on the armature—that is to say, about 15. The effect of these constant-load magnets is to make the lead perfectly constant for all currents, and the non-sparking position of the brushes extremely broad. By properly placing the brushes a very considerable compounding effect may be obtained. That seems to get over the difficulty of a sparking limit in two-pole dynamos.

Now, with regard to efficiency, it seems as if multipolar dynamos could hardly compete in efficiency with two-pole dynamos. Mr. Esson has stated that the losses by hysteresis for a given total induction will be the same in the two cases, because, though the number of reversals will be increased in proportion to the number of poles, the weight of iron will be diminished in the same proportion. Now the weight of iron will not be diminished in quite the same proportion, for by increasing the size of the central hole the weight is not

diminished in the same proportion as the section; therefore <sup>Mr. Housman.</sup> hysteresis will be considerably greater in the multipolar dynamos than in the two-pole. Then the Foucault currents will be nearly proportional to the number of poles. Therefore it seems impossible to obtain so high an efficiency in an armature with multipolar dynamos as with two-pole, though, owing to the excellent ventilation, the temperature may be kept down to the same point. The magnets will hardly be so light as stated, because stray field would increase with the number of poles, and of course the economy in magnetising force obtained by having all the wire on a single magnetic circuit is all in favour of two-pole dynamos. There is one other difficulty that comes in in two-pole dynamos if they are of large size. That is, the number of sections for a given voltage becomes very small, and owing to the large self-induction of each coil the energy available for the spark becomes very unmanageable; but this seems as if it could be got over by a plan which Dr. Hopkinson suggested some years ago of winding the armatures with multiple circuits, each circuit quite distinct, and connected to alternate bars of the commutator, the brushes being so thick as to always cover one section of each circuit. The advantage obtained is the same as that of reducing the number of turns per section in smaller machines.

Mr. SWINBURNE: Electrical engineers are indebted to Mr. Esson <sup>Mr. Swinburne.</sup> for going carefully into the comparison of two-pole and multipolar dynamos. He has broken new ground, and opened up a field which is especially important in connection with central station dynamos, whose size and number are continually increasing.

The whole question of maximum ampere-turns possibly may, I think, be put very simply. Suppose, for a moment, that the field magnets are made of such very soft iron that it takes no excitation to magnetise them; and suppose, for the moment, there are no back ampere-turns on the armature. The forward induction under the trailing pole corner is exactly neutralised by the cross induction of the armature, when the ampere-turns under the pole-pieces are equal to the ampere-turns on the field. There is then no margin to reverse the section. As iron is never infinitely permeable, so ampere-turns have to be added to the

Mr.  
Swinburne.

field excitation to magnetise the iron, and, if the brushes have a lead, ampere-turns have to be added to the field to compensate for the back ampere-turns on the armature.

Mr. Housman has objected to my statement that a section needs a very small field to reverse it. I am not sure that I quite follow his objections, as I regret to say I was not listening; but I think he said he had tried experiments with a ballistic galvanometer, and with a brush of varying width. The ballistic galvanometer would give too much self-induction, as it includes the circuit marked 5 in my paper.\* The brush experiment tells us little unless we have full particulars of the machine and the angles of lead. I think it is also possible Mr. Housman has been experimenting on small air space machines. In that case, the method followed out on pages 98 to 100 in my paper gives a much larger self-induction. This is fully recognised on page 114 of the paper referred to, and in a patent for small air space machines with small magnets in 1886. Sparking is by no means the only consideration in comparing two-pole and multipolar machines. I do not think Mr. Esson has laid enough stress on two other most important points. If a two-pole drum is re-designed as a four-pole machine, the end connections are enormously simplified, and an armature is produced which is not only easily repaired, but unlikely to need repair. In addition to this, a large clear space for ventilation can be allowed through the armature, as the ends do not obstruct the draught, and the radial depth is less. It is on these grounds that I have so strongly advocated the use of four-pole drum machines. This type of machine has been particularly successful in the hands of Mr. Crompton. I can see no reason for going beyond four poles, as a four-pole drum can be made to give a million watts without sparking, and the armature is not improved by using six or eight poles, and the field excitation is increased without corresponding difficulties.

The use of parallel winding of multipolar armatures should be carefully avoided. Suppose, for example, a large station multipolar machine is made with the armature coupled in

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\* *Journal*, 1889, page 98.

parallel; and suppose, for example, that the air space is one <sup>Mr. Swinburne.</sup> inch, and that the armature loss by resistance is 1 per cent., and that each part of the armature gives 100 volts and 1,000 amperes. Such a machine would probably have a 6-inch or 7-inch shaft. In a very short time the bearing will get, say, .01 inch out of truth. The E.M.F. on one side is then increased 1 per cent., and it is decreased 1 per cent. on the other, so that the armature electro-motive forces are 102 and 100, instead of 101 and 101. One side does all the work, and the armature loss is doubled. This effect is lessened to some extent by the back induction, and the magnets chiefly affected have been taken; but, on the other hand, .01 inch is a small amount for a large, heavily loaded, and fast-running bearing to settle down. Similar waste and heating, as well as serious sparking, are produced by any want of truth or symmetry due to inaccurate workmanship, or want of homogeneity, in cast-iron fields. I believe this point has not been brought forward before; but it seems to me to be of great importance; in fact, one hardly knows what will be the fate of the numerous parallel machines now in use when they have been worked for some time. Many engineers have been working out series windings for drum armatures. The first clear exposition of the method of winding in series is, I think, due to Mr. Fritsche.

Mr. HOUSMAN: Supposing you take a dynamo and run it as a <sup>Mr. Housman.</sup> motor on no load, and measure the current required to drive it at different volts with constant excitation of the field. Then, by plotting the experimental points, a curve is obtained which takes the form of a straight line sloping upwards. Taking any point on that curve, the interpretation of it is that at this particular number of volts the sum of the losses by hysteresis, friction, and Foucault currents is the product of the co-ordinates of the point.\*

Now it should be seen that that product represented by that area is made up of two parts. One part is proportional to the volts (or speed), and the other part increases as the square of

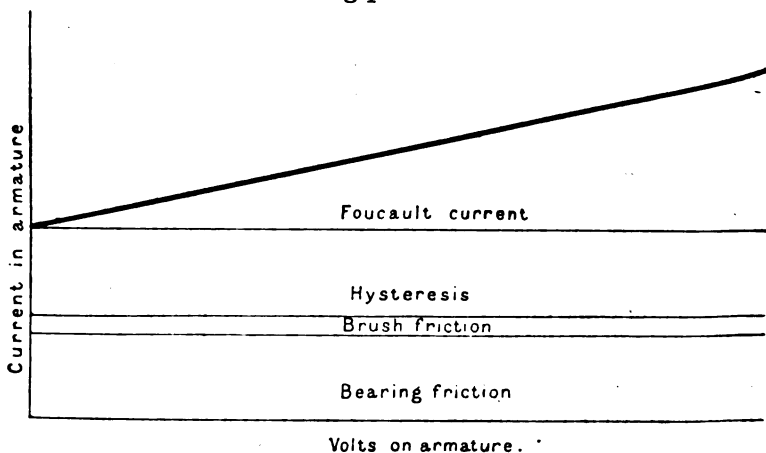
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\* For graphic method of analysing losses in armature cores, see *Electrician*, April 10th, 1891.

Mr.  
Housman.

the volts (or the speed). So that the diagram is divided into two parts, one of which is proportional to the speed (hysteresis and friction), and the other proportional to the square of the speed (Foucault currents). In order to separate hysteresis from friction you have to make a separate experiment on the power to drive the dynamo when it is not excited. This may be done by driving the machine by another motor and observing the increase of power when the dynamo to be tested is coupled on. From this the current required to overcome friction may be calculated, and will be found constant for all speeds (see figure below).

Brush friction may similarly be measured and represented by a separate area. Now, supposing this experiment is with a different strength of field, it will be found that a second curve is obtained above or below the first, according as the sum of friction and hysteresis is increased or diminished (there is a certain intensity for which this is a minimum). The loss by Foucault currents for given volts is constant, which is shown by the curves for all intensities being parallel.



Mr. Snell

MR. ALBION T. SNELL: Mr. Esson's paper has raised many practical points which perhaps are well known to the workshop designer, but are not so well understood by electricians not engaged in such work.

Mr. Esson has used the term "volume of current." Well, this may be open to some objection; but, on the whole, it is

undoubtedly a quantity most generally considered by designers, Mr. Snell. and hence has a distinctly understood meaning, and moreover we can from it at once calculate the current-density in the armature. Mr. Esson expresses the ampere-turns and the volume in the following convenient formulæ:—

$$\text{Ampere turns} = \frac{W C}{p^2}, \text{ and volume} = \frac{W C}{p},$$

where  $W$  = turns counted all round,  $C$  = total current,  $p$  = number of poles in field.

He also gives an equation to determine the volume in terms of the diameter of the armature. He gives for cylinder armatures the volume as 400 times the diameter, and for drum armatures the volume as 600 times the diameter, the armature dimension being in centimetres. My own experience coincides strongly with this, and I propose later on to put on the board some figures showing the output with a given armature with two poles, and the same sized armature with four poles.

Another equation mentioned is one for output of dynamos. This involves the square of the diameter of the armature, its length, its number of revolutions per second, and a constant. Mr. Esson for cylinder armatures finds this constant to be .048, and for drum armatures .072. Some two years ago I had occasion to devise an empirical equation for rough work in the shops, and I adopted one of somewhat similar form. I took the square of the diameter of the armature, the length, and the speed in revolutions per minute. To make the equation suitable for ordinary workshop practice, I took the inch as the unit of measurement, and fixed the magnetic density in the armature instead of in the gap, as Mr. Esson does. My constants consequently differed from Mr. Esson's, but bore the same ratio precisely. Mine are .01 and .015; Mr. Esson's are .048 and .072; the ratios are precisely the same in each case; my equations give lower outputs than Mr. Esson's, because I selected a lower density. I think the assumption Mr. Esson makes of fixed magnetic density in the air gap is more correct if you wish to consider the sparking limit, but if we wish to determine the output of the armature we should, I think, consider the magnetic

Mr. Snell.

density in the core. Mr. Esson rightly points out that in theory the output of a given armature, considered by itself, is independent of the number of poles in the field; but in practice, owing both to cost of construction and the disturbing effects of cross magnetism, I do not find this to obtain. If the armature is above a certain diameter, the output can undoubtedly be raised by increasing the number of poles. I will put on the board a few figures to show this. The armatures I am referring to are 60 centimetres in diameter, and they were designed with two poles and four poles respectively. The volume of the two-pole machine is 26,600, and that of the four-pole machine is 14,700. I may just say that these figures are approximately what Mr. Esson's equations would make them.

The watts given by the two-pole machine are 50,000 at a certain speed, and with the four poles 75,000 at approximately the same speed; the air gap and the heat waste are about the same in each machine.

In the two-pole machine the current is 70 amperes and the volts are 700, and in the four-pole machine the output is 300 amperes and 250 volts. The reason for the difference of outputs is a stronger field in the four-pole machine.

Now the advantages that I conceive can be obtained from a multiple field are these. We can have relatively a small gap for a given size of armature, and the cross magnetism is very little. We can make the magnets light, and we can get relatively high peripheral speed of armature; and, moreover, the small cross section of armature core lessens the cost. Now all these points seem to me to indicate an increased output for a given size of armature, and a decreased cost of construction. Practice enables me to say this, because I find in my own experience that with a large armature, about 55 or 60 centimetres in diameter, the two-pole magnets are costly, both as regards iron and copper.

There are these disadvantages with multipolar machines: we require a very high magnetic density both in field cores and in the gap. All the calculations I have made show a relatively high magnetic density both in field and gap, and unless great care be taken in designing the shape of the field,

the copper required to give the necessary magnetic density in the gap will be excessive. Considering these points, I believe the best forms of fields to be those in Figs. 4 and 7 (*vide* Mr. Esson's paper). I have not built machines like these, but I think them to be superior to the design shown in Fig. 6. The quantity of copper in Fig. 6 is of course greatly in excess of the quantity of copper in Figs. 4 or 7. By using wrought-iron cores and polar projections we can make the quantity of copper used a minimum, at the same time keeping the density in the gap as low as possible. I believe that on the Continent machines have been made with very long polar projections (Fig. 7), in order to reduce the density in the gap; but I do not know that there is any gain beyond a certain point; probably there would be leakage at the tips. I think Mr. Esson's paper has materially advanced our knowledge on the subject.

Mr. L. B. MILLER: There are one or two other points which Mr. Miller Mr. Esson has not considered. One is the part the volts play in deciding whether the machine shall be multipolar or not. If he had to construct a machine for 2,000 amperes and 50 volts, he would probably make a multipolar dynamo; but not if the output were 2,000 volts and 50 amperes. In the latter case he would make a two-pole machine, to save trouble with the commutator. It would be difficult to make a multipolar machine, because there must either be a great many sections, or a great many volts between the sections. Then there is the question of the utility or otherwise of cross connections in multipolar machines between sections of the commutator, which should be at the same potential, but which might differ slightly, owing to various irregularities. I have worked two similar machines, one of which had cross connections inside the commutator and one had not, and I am decidedly of opinion that cross connections are worth the extra expense. I ought to note that Mr. Esson has not referred at all to unipolar machines, presumably because he thinks they are not worth anything. But I think that is a mistake. I know that one of the faults usually attributed to them—that there is a great difference in the volts measured at

Mr. Miller. the terminals when working on open circuit, and when fully loaded—does not exist if they are properly designed.

Mr. R. E. CROMPTON: Mr. Esson's paper contains an amount of information particularly useful to engineers engaged in the construction of dynamo machines.

Several speakers have disagreed with him when he points out the economy we obtain by increasing the number of poles from two to four; my own experience coincides with that of Mr. Esson. At our works we have made a considerable number of large four-pole machines, and those made latterly have been of the highest—namely, about 95 per cent.—commercial efficiency.

We find that the reduction in material when a four-pole machine is compared with a two-pole machine, at the same output and the same number of revolutions per minute, is as 5 is to 4; and it is quite possible we shall increase this difference.

Mr. Swinburne has called your attention to the advantage of coupling up the winding in such a manner that the inductive effect of the poles acts on the winding in series instead of in parallel. This method, although I am told it has been frequently used, was first brought before me by Mr. Fritsche, the inventor of the now well known iron wheel dynamo.

In addition to the advantages mentioned by Mr. Swinburne of preventing the circulating currents that he mentions, it has the further advantage that the number of sets of brushes is reduced to two, which makes the construction of the brush-holders simpler, and the maintenance of the brushes themselves an easier matter. This mode of construction also allows us to make multipolar machines of high E.M.F.; in other words, we can get high E.M.F. by increasing the diameter and while keeping the number of revolutions small.

A ballot for new members then took place, at which the following candidates were elected into the Institution:—

*Foreign Members:*

D. Göllner.

| J. D. Pasteur.

*Members :*

Bertram Robert Beale.  
T. D. Berrington.  
Archibald Denny.  
Druitt Halpin.

Mark Henry Hurrell.  
His Grace the Duke of  
Marlborough.

*Associates :*

Archibald Campbell.  
F. Heath Deeley.  
John Attree Dumbrell.  
Viggo Gandil.  
W. Gibbings.

Arthur William Jackson.  
Andrew McGeoch.  
Thomas Lodwick Miller.  
Frank Raper.  
F. W. Smith.

*Students :*

Thomas Beveridge.  
James R. Craddock.  
John D. Fletcher.  
James Hugh Garrett Gandy.  
Charles Frederic Ratcliffe.  
George James Rogers.

John Kendall Stothert, B.A.  
Walter Edward Toy.  
Arthur Fox Ward.  
Norman Ward.  
Charles Courtenay Wharton.

The meeting then adjourned.

The Two Hundred and Twenty-second Ordinary General Meeting of the Institution was held at the Institution of Civil Engineers, 25, Great George Street, Westminster, on Thursday evening, April 23rd, 1891—Professor AYRTON, F.R.S., Vice-President, in the Chair.

The minutes of the Ordinary General Meeting held on April 16th were read and confirmed.

The names of new candidates for election into the Institution were announced and ordered to be suspended.

The CHAIRMAN: We shall now be happy to hear any further remarks on Mr. Esson's paper on "Multipolar Dynamo Machines," the discussion on which was again adjourned at the last meeting, by request of the majority then present.

Mr. Sayers.

Mr. W. B. SAYERS [*communicated*]: Mr. Esson makes no mention in his paper of machines with slotted armatures. I hope that a few remarks on the bearing which some of his considerations have upon such machines may not be inopportune. I refer especially to his treatment of "Prevention of Sparking in Machines generally." The mistake has been made by all, or nearly all, designers up to the present, of considering the use of slotted armatures as a means whereby to reduce the excitation necessary for the field magnets by reducing the resistance of the air space. Thus, Mr. Swinburne in his paper last session referred to machines of this class as "short air space machines," and made a rather obscure statement upon the effect which the short air space had in reducing the weight of the magnets.

It is perfectly clear, however, that the ratio of the magnetising force of the back and cross turns, respectively, of the armature to that of the field coils must have a limiting value, irrespective of the air space, if too great distortion, and consequent sparking and falling off of E.M.F., with moderate loads, are to be avoided. If, with a short air space, a sufficient magnetising force is put on to avoid distortion and sparking, the result is that with light loads a far greater induction is reached in the armature core than is

desired; the E.M.F. falls off rapidly as the armature current is increased, and the result is generally unsatisfactory. The air space, therefore, with a slotted armature (if the latter is properly designed), should be the same as with a smooth core. The radial depth of the core is counted from the bottom of the slots, obviously, and the air space from the top of the teeth to the surface of the pole-piece. Thus a slotted armature is somewhat larger in diameter than a smooth core, and Mr. Esson's figures would require some modification before using them in connection with slotted armatures. I may here ask Mr. Esson to be kind enough to state in his reply whether his  $d$  (diameter of armature) is taken over all, or whether it is the diameter of the core only. The *sine quâ non* of a good slotted armature is, that the sum of the areas of the cross section of the teeth, under one pole-piece at any time, at their smallest part, must not be less than the radial section of the core. (The use of armatures having these two quantities equal was introduced by Mr. George Hookham, of Birmingham—I think in 1883.)

On account of the increased self-induction of each coil, due to the wires being nearly surrounded with iron, a somewhat stronger field is required to effect the reversal of the current in the short-circuited coil, and this means a greater lead must be given to the brushes than with a smooth-cored armature, unless special means are adopted to produce a reversing E.M.F.

Some of the obvious advantages to be obtained by the use of slotted armatures are—securing a positive drive without having resort to the objectionable practice of disturbing the symmetry of the winding by the use of driving projections between the coils at intervals; the reduced liability to damage when the armature has to be removed for repairs or examination; and, with a proper air space, the increased clearance, and consequent improved ventilation, owing to the air space not being occupied by the conductors. I hope at some future time to have an opportunity of bringing the results of my experience with machines of the class I have referred to before the Institution in a more detailed and exact manner. Meanwhile, my object has been to point out that Mr. Esson's formulæ, although he

Mr. Sayers. appears to have considered smooth-core machines exclusively, apply in the main to properly designed machines with slotted armatures.

Mr.  
Atkinson.

Mr. LLEWELYN B. ATKINSON [*communicated\**]: Mr. Esson's paper forms a continuation of the one read by him during last session, and, like that paper, is one worthy of himself and of the Institution. The value of such communications, however, lies not so much in the formulæ, or even in the numerical data with which they abound, as in the clear lines of thought they suggest; and Mr. Esson himself rightly gauges this when he speaks of them as "clearing the ground." Some apology is perhaps necessary from me before criticising the statements and the results arrived at by so careful a student of the dynamo as the author of this paper, seeing that my own sphere has been rather in certain applications of the dynamo and motor than in the proportioning of the parts of such machinery, and it is therefore under cover of such apology that the following criticisms are made.

In dealing with the proportioning of dynamos and motors it seems to be frequently forgotten that it is very rarely that the most advantageous proportions can be adopted, and that for a variety of reasons. It is necessary to put a motor, say, in a launch. The armature diameter is settled, not by a formula, but by the fact that there is only, say, 5 in. between the shaft centre and the boards at the bottom. A motor is fixed to a coal-cutting machine. The armature is 12 in. long; 18 in. would have been better, but there is not room in the working place. A dynamo is built with an armature 3 ft. 6 in. long; 4 ft. 6 in. gives a nearer approximation to the correct design, but the engine maker has a bed-plate pattern to suit the former. And so on. Well, for these reasons we cannot expect to see all dynamos, all motors, approach the theoretically correct limits; but, nevertheless, the advantage of knowing what is the correct direction to work in is great.

Mr. Esson marks a new departure in the language of dynamo

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\* This communication was received too late to permit of Mr. Esson referring to it in his reply.—Ed.

design by introducing a new term to denote the total magnetising envelope on the armature. The word *volume* is what he chooses. The advantage of some other term than simply *ampere-turns* is clear; but the word *volume* conveys ideas not intended, and hence is to be deprecated, and, in addition, it is dimensionally incorrect. Volume, being an area multiplied by a length, cannot correctly convey an idea corresponding to an ampere-turn. We look at a river; we speak of the volume of water flowing. We mean inferentially the cross-sectional area of the stream multiplied by a length, and the length is the distance the water travels in some time we have in mind. Hence *volume* in this case is justifiable. A better word, it appears to me, to denote the total magnetising envelope reduced to one turn would be the *layer*, or *stream*.

Mr. Esson refers to the margin over and above the calculated safety sparking limit which it is desirable to allow. In this connection it is interesting to note why a limit must be allowed. One of the principal reasons is want of symmetry, or want of absolute equality in the different sections of the armature. These are caused in drum-wound armatures by unequal lengths of the winding, in all armatures by imperfect joints, and by want of symmetry in coil position, due to driving horns, &c., on the armature surface.

Curiously enough, no mention is made of the number of commutator sections. Theoretically, of course, it should make no difference what the number is; but practically it does make a considerable difference. The reasons principally are—that unless the number is large, absolute inequalities in the current ensue, causing sparking; and, further, that if sparking occurs from a want of symmetry, by increasing the number of sections the actual value of the unsymmetrical quantities are smaller.

To sum up Mr. Esson's paper, his contentions are these. It makes no difference to the output of a given sized armature whether you put it in a single or multiple field. It does make a difference with minimum clearance as to whether it sparks or not, and the comparison is in favour of the multiple field. Now, notwithstanding this, it may be generally admitted that

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multipolar dynamos are nearly always sparkers. To consider why, let us think what are the methods of arranging the brushes. The Continental practice has generally been as many pairs of brushes as of poles; the English practice, one pair of brushes, and either series-wound armatures or internal armature connections.

If one pair of brushes are used, we are confronted with the following point:—In passing from pole to pole into a similar position in any dynamo, the direction of current in the whole armature is reversed, and, moreover, is reversed in a series of steps. The number of steps depends on the number of commutator sections, not in the whole armature, but *from brush to brush*. Hence, for the same total number of commutator sections, with only one pair of brushes, the magnitude of each step in the process of reversal is directly as the number of poles.

In cross-connected armatures the unequal length of half of the number of turns adds to the difficulty. These disadvantages cease when the brushes are equal in number to the poles, but in this case the question of balancing electro-motive forces in all the coils appears.

Some Continental makers of multipolar dynamos—notably in the cases of Siemens & Halske's internal pole dynamo, and Frische's ring armature dynamo—have enormously increased the size and subdivision of the commutator; but in a machine recently designed in England by a well-known designer this point seems to have been overlooked.

I think we all owe Mr. Esson our thanks for his continued efforts in improving our knowledge of the scientific and practical details of dynamo design.

Mr. Esson.

Mr. W. B. ESSON: The first speaker—Professor Thompson—objected to the term *volume*, and remarked that it had not been taken as equal in value to the ampere-turns, which, considering the armature as an electro-magnet, constituted the winding. Of that I am perfectly well aware. It was simply because it was *not* ampere-turns that I called the particular quantity referred to, the volume, and this is not twice the ampere-turns save in the case of two-pole machines. In four-pole machines the volume is four

times, and in six-pole, six times the ampere-turns. The total Mr. Esson. ampere-turns on the armature we are not concerned with at all, but simply the ampere-turns under each pole-piece. These are  $\frac{v\phi}{360}$ , as I have stated. The total ampere-turns on the armature are, of course,  $\frac{wC}{p^2}$ , of which the fraction  $\frac{\phi p}{360}$ , under the pole-piece, gives  $\frac{wC}{p^2} \times \frac{\phi p}{360}$  as the ampere-turns reacting on the field. This is what I have got, but to shorten the work the quantity  $\frac{wC}{p}$  is called the volume throughout, and indicated by the letter  $v$ . If Professor Thompson thinks it better to keep to the more cumbersome expression containing ampere-turns, he is at perfect liberty to do so. It is, after all, a mere question of convenience in calculation; though I hold that in the form given in the paper the equation is more convenient for my purpose, if not for the Professor's.

The next point is with reference to the comparison instituted between Figs. 3 and 4. Professor Thompson suggests that instead of the single-magnet two-pole field shown in Fig. 3, a double-magnet field, notched at the top, ought to be compared with the four-pole field (Fig. 4), since we should have then the same heating surface, the same gap, the same total polar surface, and the same output. Whether the comparison suggested by Professor Thompson is permissible depends upon the size of the machine. When dimensions are reached such that with two poles the gap required to prevent sparking would be greater than that necessary for the conductors, we, of course, go to four poles; but there is still the alternative that the two-pole construction may be retained if the machine is made with a double horse-shoe magnet, practically split where the two similar poles abut. That is where Professor Thompson's comparison becomes allowable; but it would be a mistake to compare a small double-magnet two-pole machine with the four-pole one, for the single-magnet being the better construction, it should be employed for two-pole machines until their dimensions render it prohibitive.

Professor Thompson thinks that the proportion of copper to

**Mr. Eason.** iron in the alternator field shown in Fig. 2 is "prodigious;" but, as a matter of fact this proportion may be anything, depending upon the efficiency of the machine. Mr. Mordey's machine having the copper to iron as 1 to 16 may only mean that the iron part is very heavy; anyhow, what are directly comparable in Figs. 1 and 2 are the amounts of iron and copper in the two fields for the same output and efficiency, and I believe my figures for these quantities are correct. Figs. 1 and 2 represent the same class of field, though one armature gives a direct and the other an alternating current. I fail to see where any basis can be found for comparing with Fig. 1 a machine of the Mordey type belonging to an entirely different class; and so far from Fig. 2 field being obsolete, there are being made at the present time at least 50 machines of that type for every one of the kind the Professor mentions.

The two fields of Figs. 1 and 2 could not have been better chosen to illustrate a point emphasised in the paper later on—viz., that the mere adding of poles gives no structural advantage unless the magnetising force required for each air gap is at the same time reduced. In increasing the diameter of the armature we eventually reach in two-pole machines a size (from 50 to 60 centimetres) in which the air gap required to secure a fair margin as regards freedom from sparking becomes excessive, and then it pays to re-design the machine, making it of the four-pole type, with the armature dimensions modified to suit.

Mr. Kapp refers to the greater dissipation of heat consequent on the increased armature velocity in multipolar machines, and this point certainly deserves attention. I have made some experiments at odd times with a view to determine the cooling effect due to velocity, but I regret to say that they have not been carried out with the care which would enable me to quote the results with very much confidence. The relation appears to be, for smooth armatures, something like,

$$C^{\circ} = \frac{55 W}{S (1 + .00018 V)},$$

where  $C^{\circ}$  is the rise of temperature over the surrounding air in degrees centigrade,  $W$  the watts expended,  $S$  the radiating

surface in square inches, and  $V$  the velocity of the armature in feet per minute. The formula, though deduced from rough experiments, and possessing some uncertainty as to the limits of its application, may be relied upon, I think, within the limits of velocity and cooling surface met with in practice.

I quite agree with Mr. Andersen that the number of the commutator sections influences very materially the question of sparking; but what I wanted to point out was that unless certain relations were observed between the various dimensions of the machine, sparking would result, whatever the number of sections. I do not fix the brushes "in the vicinity of the pole-tips," but merely assumed that position for the purpose of calculating the critical relation, as regards sparking, between the several quantities. Having specified the critical value for the volume in terms of the induction, air gap, and pole angle, I mentioned that in practice the armature load rarely exceeded half this limiting value, and then, having embodied in the equations the results of practice as regarded the relation of diameter and volume, proceeded upon the basis so established to compare the several types of machines. I can only regret now that, so far as Mr. Andersen is concerned, my arguments have failed to carry conviction with them.

The problem which Mr. Housman considers with reference to the heating of the conductors is not similar to the case dealt with in the paper, so I need not consider it here. The experiments described to determine the magnitude of the reversing field, however, deserve the closest attention, and I mean to read Mr. Housman's speech carefully when in print.

Mr. Snell instances the case of two machines with armatures of the same size, one having a two-pole and the other a four-pole field. He remarked that the latter gave 50 per cent. more output than the former, though it had only 56 per cent. of the volume of the two-pole. These figures show that the induction was very much (about 2·7 times) greater in the four-pole field, and that the two-pole machine with a better field might have had its output greatly increased. I don't see why the two-pole machine should not have had as high an induction as the four-pole,

Mr. Eason.

especially as the air gaps were similar; and with the greater volume it ought in this instance to have given a higher output than the four-pole, provided, of course, that the volume mentioned could be carried without sparking. I don't think it matters in the least whether one considers the induction in the air gap or in the armature core; but if an expression containing the radial depth of the core, and the induction in it, is preferred, it will be found in equation (2) in the paper.

A touch of humour is apparent in Mr. Miller's lament that a paper dealing with multipolar dynamos takes no notice of *unipolar* ones; but for all practical applications which occur to me at the moment, the type of dynamo misnamed "unipolar" is, I think, a mistake. However, Mr. Miller may have something in his mind I am not cognisant of. Referring to his remark about the permissible difference of potential between commutator segments, I may state that in our arc lighting machines working up to 1,200 volts no difficulty has yet been experienced with the commutators, although the mean difference of potential between adjacent segments is never less than 25 volts. I see no difficulty in making a multipolar machine to give 50 amperes and 2,000 volts, though, of course, the design would have to be carefully considered. I may mention here that the first multipolar machine with the windings connected in two parallels for high potential which came under my notice was designed by Elphinstone & Vincent in 1884. It was a six-pole machine with the armature sections connected up in two parallels for 600 volts. But it is now broken up.

The saving in material mentioned by Mr. Crompton as due to the four-pole construction agrees fairly with what I give; and here I may say, replying to a question of Mr. Kapp which I had forgotten, that on account of the longer yokes, Fig. 6 field, if made of the same material, is heavier than Fig. 4 field, in the ratio of 4 to 3. Mr. Sayers' remarks respecting slotted armatures I endorse. For several years my firm made machines with slotted armatures, but found, when large sizes were reached, that the air gap had to be increased to prevent sparking, as Mr. Sayers states, also to prevent the heating of the pole-pieces. It

was then that we put the conductors in the space so necessitated, Mr. Eason saving, consequently, the labour in milling out the slots, and reducing at the same time the weight of the core.

In conclusion, I must thank members for receiving the paper in the way they have done, and for the many friendly criticisms offered upon it.

The following paper was then read:—

### A FEW CALCULATIONS ON ELECTRICAL SHOCKS FROM CONTACT WITH HIGH-PRESSURE CONDUCTORS.

By Major P. CARDEW, R.E., Member.

In connection with the public supply of electrical energy in which very high pressures are used, it is important to investigate the amount of shock to which persons may be exposed by contact with one conductor under various conditions of insulation and electrostatic capacity of the conductors. Serious attention is now being directed to the effect upon the human organism of shocks with steady, intermittent, or alternating currents, and the necessity for exact electrical measurement in such investigations is fully recognised.\*

The present opinion upon the subject may be stated to be, roughly, as follows:—The effect of an electric shock produced by the passage of a current through the body during an appreciable time is probably proportional to the square of the current, as regards injury by destruction or heating of tissue, with all kinds of currents; while alternating and intermittent currents produce, in addition, effects upon the nerves and nerve-centres which may be directly injurious in themselves, and are generally indirectly so by producing temporary paralysis, and thus depriving the recipient of the power of terminating the period of contact.

The resistance opposed by the body to unidirectional and to alternating currents of the ordinary frequencies appears to be different, at any rate for low electrical pressures, that with the former being considerably the higher; but this difference

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\* *Vide* the papers by Dr. W. H. Stone, *Journal of the Institution*, vol. xiii., p. 415, and Mr. Newman Lawrence and Dr. Harries, vol. xix., p. 290.

diminishes as the pressure is increased, and is probably an effect of polarisation in the tissues.

This resistance is also largely affected by the extent of surface of the body through which the current enters, and the condition of the skin.

Without dealing further, however, with the effect produced, the following calculations merely give the current, or discharge through the body, under different conditions; the electrical impedance to the flow to earth through the body, including the resistances at the point of contact and in the earth connection, being taken as a constant resistance— $r$  for continuous, and  $r'$  for alternating currents. The charging source is considered to be in all cases perfectly insulated, of negligible electrostatic capacity, and of ample power to maintain the pressure under all conditions.

The principle upon which the following calculations are based is, that every electrical machine must at any instant, or during any time, however extended, produce equal quantities of what used to be called “positive” and “negative” electricity.

This fact is as true of a dynamo as of a piece of sealing-wax and its rubber; the emission from the source, whether it be a steady current or a small charge, is equal from the positive and negative pole. Thus, if one pole be connected to a conductor of large capacity, it will necessarily be at a potential nearer to the earth than the other pole, if the latter be connected to a conductor of smaller capacity, and the whole system thoroughly insulated; and in every case this principle determines the potentials from earth of any electrified system, leaving out of question any extraneous charge imparted to it.

The first case to be considered is that of two perfectly insulated conductors, A and B, of considerable cross section, and capacities  $K$  and  $K'$ , kept charged to a steady difference of potential  $V$ . The mutual electrostatic induction is assumed to be negligible, and in general will be so, as the conductors are separated by some distance.

The potential of the earth will differ from that of A by an amount  $\frac{K' V}{K + K'}$ , and of B by an amount  $\frac{K V}{K + K'}$ , and there will be equal charges on the conductors.

If now a person making good earth with some part of his body touches conductor A with another part, the transmission through him will be not only the charge on A, but the equivalent of the opposite sign to the additional charge flowing to B to raise its potential to V. Thus the quantity that will pass through him will be

$$\frac{K K' V}{K + K'} + (K' V - \frac{K K' V}{K + K'}) = K' V.$$

The emission from the source being again equal in the two conductors, and amounting to

$$K' V - \frac{K K' V}{K + K'} = \frac{K'^2 V}{K + K'}$$

the maximum current through the body will be

$$\frac{K'}{K + K'} \times \frac{V}{r}.$$

Similarly, if contact be made with B, the quantity in the discharge will be K V, the emission from the source  $\frac{K^2 V}{K + K'}$ , and the maximum current

$$\frac{K}{K + K'} \times \frac{V}{r}.$$

Thus it appears that where the capacities of the charged conductors are unequal, contact with the conductor of least capacity will produce the greatest shock, the quantities passing through the body, and currents, being in the inverse proportion of the capacities in the two cases.

This case is perhaps not very practical, as electric supply mains, unless one was an aerial conductor and the other buried, would be, as a rule, equal in capacity. Also, there would be in any length of mains great probability of sufficient leakage to determine the potentials, irrespective of capacity, with continuous currents.

Secondly—Let the conductors A and B be concentric tubes, as in the Ferranti mains, B being outside and entirely enclosing A, except at the two ends, where the conductors are connected to the generator and receiver respectively, or to the generator at one end and the other insulated, which is the simpler case to consider.

In this case there will be no inductive action between the conductors and the earth with the conditions postulated for the generating source, and in the absence of any extraneous charge, and the potential of B with respect to the earth will be zero.

For the quantities on A and B must be equal, and no free charge can be given to B by the source unless it is leaky. If the conductors are transmitting working currents, this condition will be modified by the slope of potential on B, which will then, if insulated, be at zero-potential at the centre of its length.

The same reasoning holds with alternating currents, but in this case the tendency to low potential on the outer conductor is stronger on account of the capacity current.

In fact, with such a length of cable as is used on the Deptford trunk mains—each main having, I am informed by Dr. Fleming, a capacity of about 15 microfarads between the outer conductor and the earth—it would be necessary to have such a considerable leak on the generator or receiver at or near the point of connection to the inner conductor, to raise the potential of the outer much above the earth, that, considering the pressure on the inner conductor, a breakdown must result, and this even if there were no metallic connection to earth on the outer conductor.

It is obvious, too, that no leakage between inner and outer could affect the potential of the latter, except to the extent due to the resulting current and the resistance of the conductor.

The writer pointed out at the time that the question of the use of an earth connection on these trunk mains was first discussed, that the outer conductor would even without this connection always be at a low potential so long as the circuit was in a workable condition; and he has recently been informed that this is found to be the case at Rome.

The earth connections in use have, however, important functions as safeguards in the event of a breakdown in a main transformer.

It is somewhat curious to note that the charge on the outer conductor of the Deptford trunk mains at any instant does not come from the terminal of the dynamo in connection with this conductor, but from that connected to the inner conductor, and passes through the transformer.

Thirdly—Let the capacities of A and B be negligible, but their insulation be no longer perfect, the insulation resistance of A being  $R$ , and of B,  $R'$ .

Then with a steady P.D. the potential of A will be  $\frac{R V}{R + R'}$ , and of B,  $\frac{R' V}{R + R'}$ , on the principle of equal emission.

If contact be now made with A, its potential is at once reduced to

$$\frac{r R V}{r R + R' (R + r)},$$

and that of B increased to

$$\frac{R' (R + r) V}{r R + R' (R + r)};$$

the current through the body being

$$\frac{R V}{r R + R' (R + r)}.$$

If the conductors have appreciable capacity as well as leakage, there will result in every case a discharge through the body due to the rearrangement of potentials with respect to the earth, since the effect of the contact must be to lower the potential of the conductor touched.

Fourthly—The general case with leakage and capacity with alternating currents.

This requires working out. Let, as before, A and B be the two conductors between which an alternating pressure, maximum  $V$ , is maintained at a frequency  $n$ ; and let them be considered to be separate conductors with negligible mutual electrostatic induction.

Let  $K$  be the capacity,  $\left. \begin{array}{l} \\ R \text{ insulation resistance} \end{array} \right\}$  of A.

$K'$  capacity,  $\left. \begin{array}{l} \\ R' \text{ insulation resistance} \end{array} \right\}$  of B.

$V_a$  maximum pressure of A from earth;

$V_b$  maximum pressure of B from earth;

$$p = 2 \pi n.$$

The current at any instant flowing to each conductor will be compounded of the charging and leakage currents respectively, and

the phase angle between the pressure and the current for each conductor will depend upon the capacity and insulation resistance, and will not be the same for the two conductors unless the product of the capacity and insulation resistance is the same for each.

But the current in each conductor must be identical in phase, and equal at any instant, on the principle of continuity.

Therefore in general there will be a difference in phase between  $V_a$  and  $V_b$ , as shown in Fig. 1.

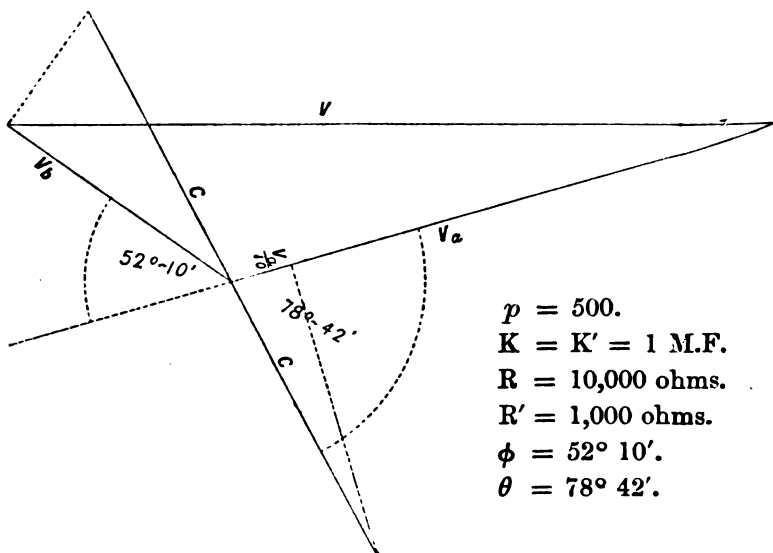


FIG. 1.

Let  $\phi$  be the angle measuring this difference; and assuming the sine law of variation, we have at any instant the potential of  $A = V_a \sin a$ , and of  $B = V_b \sin (a + \phi)$ , where  $a$  is the phase angle; and as the quantity flowing to each conductor in the succeeding instant is the same,

$$K V_a d (\sin a) + \frac{V_a}{R} \sin a dt =$$

$$K' V_b d [\sin (a + \phi)] + \frac{V_b}{R'} \sin (a + \phi) dt,$$

$$\text{or } p K V_a \cos a + \frac{V_a}{R} \sin a = p K' V_b \cos (a + \phi) + \frac{V_b}{R'} \sin (a + \phi).$$

Since this equation is true for all values of  $\alpha$ , let  $\alpha = 0$ , then

$$p K V_a = p K' V_b \cos \phi + \frac{V_b}{R'} \sin \phi \quad \dots \quad (1)$$

Again, let  $\alpha = \frac{\pi}{2}$ , then

$$\frac{V_a}{R} = \frac{V_b}{R'} \cos \phi - p K' V_b \sin \phi \quad \dots \quad (2)$$

whence, 
$$\tan \phi = \frac{p (K R - K' R')}{1 + p^2 K K' R R'} \quad \dots \quad (3)$$

The straight line joining the extremities of  $V_a$  and  $V_b$  represents  $V$  in phase and magnitude; whence,

$$V_a^2 + V_b^2 + 2 V_a V_b \cos \phi = V^2 \quad \dots \quad (4)$$

From these equations the values of  $V_a$  and  $V_b$  can be deduced in terms of  $V$ ; but as the resulting formulæ are somewhat cumbrous, it is simpler to proceed thus:—First, determine  $\phi$  from equation (3); secondly, determine  $\theta$ , the angle between  $V_a$  and  $C$ , the current, from the equation,

$$\tan \theta = \frac{p K V_a}{\frac{V_a}{R}} = p K R.$$

The directions of  $V_a$ ,  $V_b$ , and  $C$  are thus determined:—Take  $C$  of any convenient length on each side of the origin, and from the ends of  $C$  drop perpendiculars on  $V_a$  and  $V_b$ : the distances between the origin and the feet of these perpendiculars are proportional to  $\frac{V_b}{R'}$  and  $\frac{V_a}{R}$ , and may be taken as  $V_b$  and  $\frac{R'}{R} V_a$ . This determines the length of  $V_a$  and  $V$ . In the example shown in Fig. 1,  $V_b = \frac{V}{2.9}$ ; or the current through the leak  $= \frac{V}{2,900}$  maximum.

Fifthly.\*—If the insulation of the two conductors be supposed perfect before contact is made with one of them, the equations are simplified.

Let  $B$  be the conductor touched, then in the equations  $R$  becomes infinity, and  $R' = r'$ ;

\* Cf. "On an Unnoticed Danger in certain Apparatus for Distribution of Electricity," by J. Hopkinson, F.R.S., *Phil. Mag.*, Sept., 1885.



of the oppositely charged conductor, as has been shown to be the case with unvarying pressure.

Fig. 2 shows the phases and relations of the quantities,  $V_a$  in this case being in quadrature with  $C$ .

As a rule, with separate conductors the capacities of each with respect to the earth will be equal, in which case equation (5) is modified to the following:—

$$C = \frac{p K V}{\sqrt{1 + 4 p^2 r'^2 K^2}},$$

which may be written,

$$K = \frac{C}{p \sqrt{V^2 - 4 C^2 r'^2}}.$$

Supposing  $C$ , the current through the body, to have the maximum value which may be taken without injury—or, say, 20 milliamperes— $r'$  to be 2,000 ohms,  $V$  2,000 volts, and  $p$  to have the value 500, we get from this  $K$  about .02 microfarad only; which clearly shows the danger of touching bare conductors used for the supply of alternating currents at high pressure under any conditions.

The CHAIRMAN: Of course it is difficult to discuss a paper like this in detail at once, but, as we have some little time before us, some members may possibly like to offer some remarks.

Dr. J. A. FLEMING: The subject to which Major Cardew has directed our attention is an eminently practical one. Anything that can throw light upon the circumstances or conditions under which danger from shock is to be apprehended is of value. Many people are under the impression that there is something intrinsically dangerous in high pressure *per se*. Of course this is not the case. No one would object to take a discharge of several thousand volts in initial pressure, provided it came from a Leyden jar of sufficiently small capacity. In order that there may be danger from a shock, it is essential that a certain quantity of electricity should pass through the body, and it is important to know what is the least quantity which, under given circumstances, can be so passed without danger. I once had the curiosity to make experiments upon myself to discover what currents could be passed through the head or body without causing incon-

Dr  
Fleming.

Dr.  
Fleming.

venient pain. I found that more than a few milliamperes, say 5 or 6, through the head gave rise to dizziness; but that more could be passed through the body, although that with 40 or 50 milliamperes the skin rapidly became blistered if continuous currents were used. The above experiments were, for obvious reasons, made with Leclanché cells. People differ very greatly in the amount of current which they can endure passed through them without pain. I should be inclined to think that with many, or most, people half an ampere passed through any of the vital organs for more than a very short time would be fatal. One interesting question connected with this is that the human body acts both like a condenser and like an accumulator. The body, if charged by a small current sent from one hand to the other, will give a reverse current when the wires of a galvanometer are taken in the hands. It acts like a voltmeter, and can be polarised. Also, it acts like a condenser possessing a measurable electrostatic capacity. It is very probable that electrolysis of the fluids of the body is set up under the influence of continuous currents, and this may, perhaps, account for the peculiarly fatal character of the shock from some arc-light machines. Passing on to one other point in Major Cardew's paper, I can confirm, from experiment, what he says about the exterior conductor of concentric cables always assuming the potential of the earth when the two conductors are insulated and connected to an insulated dynamo. This fact is not so generally known as it should be, and it may be taken to be one of the recommendations for using concentric cables with alternating currents. In conclusion, let me say again that the question brought before us in the paper is one very deserving of consideration, and that there is need for a good deal of research to enable us to understand the circumstances under which cause of danger from electric shocks may exist.

Mr.  
Lawrence.

Mr. H. NEWMAN LAWRENCE: I am sorry to say I was unable to be present during the earlier part of the reading of Major Cardew's paper; but I have heard what Dr. Fleming has said, and I think we all owe him a debt of gratitude for the way in which he has spoken of the subject. I have been able to make a good

many experiments on the effects of shocks on different parts of <sup>Mr.</sup> the body; but, as you all know, it is not easy to get subjects <sup>Lawrence.</sup> willing to undergo experiments made with currents sufficiently strong, or similar to those used in ordinary commercial work. There are a few points Dr. Fleming touched upon I should like to refer to. First, in connection with the possible amount of current which may be safely taken through the head. Dr. Fleming assured us he was able to take 4 milliamperes through the head. I am very glad to hear that he is able to do so, but I should not care to do that, though much depends upon the direction of the current. His suggestion that possibly some could take 20 milliamperes through the head can hardly be taken seriously. The point which it is very important to bear in mind is the direction of the current. Though it might be possible to receive 4 milliamperes as a maximum transversely through the brain, one might take more longitudinally. A very small current transversely is sufficient to make one very dizzy, but longitudinally it would take three or four times as much to produce the same result, although I should not recommend it to be done. Anything above 4 milliamperes, might not, perhaps, have very serious consequences immediately, yet the after effects would be bad. Dr. Fleming has said a great deal about the effect upon the body of a possible electrolysis being set up. That, I believe, is a very serious matter, and one which ought to be, if possible, fully investigated. This leads one to remark upon the great want of information there is upon this subject, and the great difficulty there is in obtaining reliable information, after an accident has occurred. I think a great deal might be done in some way or other by this Institution in endeavouring to obtain accurate information after all accidents, in order that something like reasonable laws of probabilities under certain conditions may be found out, which should form a useful guide to those engaged in electrical work. We occasionally hear of accidents, and are told a certain number of facts in connection with them, but very often those facts are utterly insufficient to enable anything like a satisfactory conclusion to be arrived at. We may be told there were 100 or 200,

Mr.  
Lawrence.

or, it may be, 2,000 volts; but we know nothing whatever about the important conditions which regulate the way in which the current may or may not have passed through the body, or what small fraction only may have been received. The question of contact area, the condition of the skin, and a number of other things come in so largely that, without detailed information obtained on the spot, other particulars are of very little value. I do earnestly hope that something may be done by this Institution to enable us to get that information. One other point I was very glad to note. Dr. Fleming concluded by referring to the fact that it is the quantity of electricity which passes through the body that is of importance, and that it is not of much use talking about the pressure unless we know something of the quantity. To talk of pressure only is decidedly misleading.

Mr. Adden-  
brooke

Mr. G. L. ADDENBROOKE: It is possible that I can do something to supply the details asked for by the last speaker. When I had charge of the outside work of the Grosvenor Gallery, about four years ago, we had a good deal of experience of shocks. I have received more or less slight ones myself, and I have been witness to those which others have received. In the first place, I once performed the operation of jointing on the mains while the tension of 2,500 volts alternating was on them, on a pole over head. It was found an extremely difficult matter to connect customers without taking off the current, while if the current was taken off there were always very many grumblings. When thinking over the various methods which could be adopted to get over this difficulty, it struck me that it would not be such a very dangerous thing to do, to make the joint on the high-tension mains overhead when the current was flowing through them; so I determined to try it myself. The way I proceeded was this: First of all, I had a strong teak bracket made, which I could fit on the ordinary iron pole which is used for overhead work. Then I had some sheets of thin sheet india-rubber prepared, with which I covered the pole, the cable, and all exposed ironwork near the cable. The cable then, which was fastened at each pole to insulating shackles, was

separated from them, the rings holding it on the suspenders were taken off, and the cable itself made quite clear for a length of about two yards. I chose a dry day for doing this, because the outside braided covering of the ordinary cable takes in a good deal of water, and it is sometimes possible to get a considerable shock from it if it is damp. Having made these arrangements, and having instructed the men what to do in case any accident happened, I proceeded to work. First, I cut off the insulated coverings of the cables and got down to the bare conductor. I may say I had a pair of gloves to protect my hands, and also that I had tools with large ebonite guards fitted on them between the handle and the blade, so that even if one's hand slipped it was not necessary to touch the conductor. In fact, I had come to the conclusion that it was possible to make the joint without touching the conductor at all with the hands, or with anything that was not insulated from the hands. As the work progressed, however, I found the gloves inconvenient, and discarded them, preferring to use my bare hands, and while doing this, I frequently touched the bare conductor, when an intermittent spark passed—the sort of thing one might get with a small induction coil worked by a very weak battery—just enough, in fact, to show a little blue light, and to produce a moderately painful tingling sensation. Besides this, nothing particular occurred up to the point when I was soldering on the connections to the customer from the main cables. There happened to be a loop of insulated conductor overhead, which hung a little in the way. In his anxiety to assist me as much as possible, one of the men, in the endeavour to take this loop out of my way, touched the end of the stick of solder which I held in my hand, and we both felt a pretty smart shock. The conditions were such that he either drew his hand away instantly, or it was drawn away instantly by involuntary muscular contraction, so that beyond the painful sensation for the moment nothing further occurred.

Mr. Adden-  
brooke.

On another occasion, I believe I once swept my hand across a switch with a high-tension current on it; I think I must have done so, but I am not quite sure whether I did or not. It was during some alterations in the engine-room, when access to the

Mr. Adden-  
brooke.

switchboard was difficult. I had made some tests which had lasted after the usual time for turning current on. When I did so, I found that the main switch did not fit well, and that a small arc was formed. In order to remedy this, I cut a small slip of wood which I was going to put into the switch to act as a wedge. I was conscious that it was possible, standing on an iron rail as I was, and leaning over towards the switchboard, that if my hand slipped something might happen. I suppose my hand did slip; at any rate, something went, and I found myself down below. But, as far as I am aware, I did not know that I received any shock, except a general feeling of disturbance. The switch came entirely out of the contact, but I was sufficiently alive to get up and put it back again.

So far as regards my own experience. Now, turning to that of others, several of the men received more or less bad shocks, and had their fingers burnt. One case I particularly remember. A transformer was being put up in a customer's house. In order to put these transformers up it was usual to fix cast-iron brackets in the walls to support them. One of these cast-iron brackets had been fixed in a wall, and, as you will readily understand, having only been put in a day or two, the cement was wet, and made a reasonably good earth. One of the linemen who happened to be putting up the transformer was not aware that the connections were made, and, wishing to reach the bare end of a main above him, he caught hold of the bracket with one hand and reached up to the bare main connected to the 2,500-volt alternating circuit. I was not there myself at the time, and do not know absolutely what happened, but I believe all his muscles were contracted, and he could not move; however, he was pulled off by another man present, without sustaining much injury. He still lives, and has never, so far as I am aware, suffered anything particular after it. Another case of shock, which came more particularly under my own observation, was that of a man who was rearranging the customers' leads in the neighbourhood of the Grosvenor Gallery. It was a very hot summer day. This man was on a pole, probably with his legs around it to support himself. As far as I can make out, he must

have got one of the leads through his left hand, and there must have been a crack in the insulation at the joint. I think, probably—it being in the middle of summer—his hand must have been very damp with perspiration, which probably passed into the joint and was sufficient to cause an arc. The effect of the shock was that his muscles were all contracted, and he remained clinging to the pole unable to move at all. Another man on the spot with him, who saw what had happened, endeavoured to obtain assistance, but, being too far away to do so, proceeded to pull him down himself. The conformation of the roof happening to be a little awkward, the result was that he pulled the man down on top of himself, and they both fell almost 15 feet on to a tiled roof, which they broke through, and then both fell a further distance on to the floor of a loft.

Mr. Adden-  
brooke.

Professor PERRY: What was the first man touching?

Mr. ADDENBROOKE: He held the lead with one hand, his legs were round the pole, and probably the other hand was on the pole.

Professor PERRY: He was earthed, in fact. The other conductor was supposed to be insulated?

Mr. ADDENBROOKE: I expect the insulation from earth of the circuit otherwise would be about a megohm at the time. The man received a cut on the back of his head whilst he was passing through the tiled roof; in fact, the cut was so long that I cannot account for it, except by the fact that his head must have altered its angle towards the tiles as it was passing through. He was removed to the Middlesex Hospital, and I went round there to see what had happened. I saw the house surgeon, and found the man doing extremely well; I explained to the surgeon what had occurred, and said as it was an interesting case I should be much obliged if he would make any examination he thought fit and let me know the result. He replied that the man's temperature was perfectly normal, and there did not appear to be anything wrong with him in any way, nor was his heart in any way affected. On examining him, there appeared, as always appears in these cases, an effect which I can only compare with the effect of laying a red-hot poker on the skin. All the skin

Mr. Adden-  
brooke.

between the thumb and finger of the hand was charred away black, as if a red-hot poker had remained in contact with it for some time. It was a nasty sort of burn, of course, but quite clean, and it healed up in the course of two or three weeks, without any serious after-consequences. As far as I could ascertain from the house surgeon at the time, the man's constitution was in no way affected, and beyond being generally a little out of sorts, he was not otherwise the worse for it. I might give other cases of much the same sort, but these are instances which came more particularly under my own observation.

Mr. Mordey.

MR. MORDEY: I can confirm the point brought forward by Major Cardew and Dr. Fleming as to the potential difference between the outer conductor of a concentric cable and the earth. The other day, in examining some miles of underground work concentric cable, I found that the outer conductor, which was well insulated, was at the same potential as the earth, tested statically.

As to the danger in connection with high-tension work, I may mention that although at the Brush works we have been in the habit for over ten years of making high-pressure dynamo machinery, and although our arrangements are not at all elaborate—machines in the testing room being run for a few hours often on circuits made up in rather a temporary manner, and with the assistance of people who have not had much practical experience—we have had no serious accidents. We have had accidents, even fatal accidents, amongst the ordinary machinery, but have never had a serious accident from electrical machinery. I mention this as showing that in a place where the machinery is mostly high-tension, and the conditions are unfavourable, we have never had an accident of any moment. I have had bad shocks; other people in the place have had bad shocks; but I only remember one case—a good many years ago—of a man being rendered insensible by a shock, though perhaps thousands of machines—certainly a very great number—have been tested in the last ten years. In the case of the man who became insensible he was a lamp-trimmer; he was on a ladder taking down a lamp in the shop

circuit. He got the lamp off one hook, and in some way com- Mr. Marder  
pleted the circuit to the other hook. The lamp was running  
on the circuit of an ordinary No. 8 Brush machine, which gives  
2,500 volts when working at full load; it was working then at  
about 1,500 volts, but had an automatic regulator which always  
tried to give 10 amperes, whatever circuit the machine might be  
working through. The man got a very severe shock, and fell on  
to the ladder, keeping hold of the lamp. An arc was seen passing  
between the hook and his hand, and after a moment or two he  
tumbled down, and was picked up insensible, but soon recovered.  
His hand was very severely burned, and his fingers were afterwards,  
and are to the present day, rather drawn—he cannot quite  
straighten them. But as to any harm from the large current he  
received, there was nothing to show that he had received any  
permanent injury at all. The current that he received must  
have been very considerable. It is impossible to say how  
much, but from the fact that a considerable arc was observed,  
and from the deep and severe wound, I should say he received  
several amperes. This, however, is only a surmise, but I cannot  
otherwise account for the machine working through him.

I once had a shock—the worst I ever received—from a No. 7  
Brush machine, which gives about 1,200 volts. I was using the  
current to separately excite the magnets of another machine,  
and, forgetting that I was not getting the current from the  
ordinary low-tension source, I pulled a wire out, and put myself  
in the circuit. I was perfectly conscious, but could not speak.  
The brushes were flashing over, and I remember that it occurred  
to me that somebody ought to set the brushes back into the  
non-sparking place. I was not conscious of any pain. The next  
thing I remembered was that somebody went and stopped the  
machine. I went on with the work I was engaged in, and did  
not at first realise what had happened. I felt injured because  
people wanted to relieve me of the work. I wanted to join up  
the circuit again myself. I was a little foggy, and affected  
mentally, but only for a few minutes.

I do not quite know what Dr. Fleming meant when speaking  
of sitting in an insulated chair.

Ir. Mordey. Dr. FLEMING: If a man have capacity enough he may experience a shock, though not a dangerous one.

Mr. MORDEY: I think ordinary human beings of average size have not capacity enough. We have no hesitation whatever in handling 2,000- or 3,000-volt circuits with bare hands, standing on a rubber mat. I came straight to this meeting from some experiments in which, standing on a bare mat, we were, with bare hands, constantly opening and closing a 2,000-volt circuit of a 70-horse-power alternator. The arc lamps are all tested at high tensions—often nearly 3,000 volts—standing on wooden brackets, and the men walk about the floor and handle those lamps. If the circuit is not over 2,000 volts, they have no protection at all; if more, they feel a slight tingling, and sometimes use the rubber mats. I remember being present at a lecture—I think it was about 1881—when Professor Ayrton showed that it was quite possible to experience a shock standing on an insulated stool.

Professor AYRTON: The induction coil was hung up by silk cords. I do not remember whether an insulated stool was used or not.

Mr. MORDEY: Your point was that it was possible to get shocks even though insulated. Only a very small shock is felt in handling ordinary high-tension circuits, and may be due to charge and discharge, or to leakage. I remember, years ago, we had a cat at our works, and this cat used to walk about among the terminals of high-tension Brush machines when being tested. Perhaps some of you know the construction of the machine. The current from the commutator is brought down bare copper strips. On the large machines there are three commutators, each of about 1,000 volts. I think at the time we had that cat the machine only went up to about 2,000 volts; but the cat used to walk about, in and out, among the conductors, in the curious way that cats have, rubbing itself against the conductors. Its fur, probably, completely protected it. I do not know what became of that cat. It may perhaps have been volatilised, but, so far as I know, it died a natural death—was killed by a dog. Another case: We had at the works a packer, a man who used to be very

proud of showing his accomplishment in the matter of taking **Mr. Mordey**. shocks. I do not believe he ever got any shocks. He would take hold of the terminals of a Brush machine, No. 7, at full power, 800 volts, and smile. He had singularly dry, mummy-like hands, and I do not believe it would have been possible to give that man a shock.

**Professor PERRY**: Did he ever try to break the circuit?

**Mr. MORDEY**: No. We were not very scientific in those days—in 1881-82—and this man was allowed to show off his powers. I do not know what became of him; I do not think he was volatilised. As **Dr. Fleming** has mentioned, the internal resistance of the generator may have a great influence in diminishing the effects produced by high E.M.F. For instance, in all the old experiments with vacuum tubes one finds enormous pressures mentioned; but if you try Geissler tubes on even quite moderate-sized modern machines, you will find that the tubes blow all to pieces. An ordinary Geissler tube that will stand anything from a Ruhmkorff coil or a Wimshurst machine, will take a large current and smash if a few hundred volts are put on it from an alternator. The explanation is, I need scarcely say, that with the former the E.M.F. drops very much when a perceptible current is taken from it.

**Professor PERRY**: Of course one must feel himself unable to discuss a mathematical paper of this description, unless one has had 4 milliamperes through one's head; but I have a simple question to ask. I have heard several people talking in this discussion about these conductors from the same fallacious point of view, and I should like to know this: If you have your cables and your machine insulated from the earth, how is it that you insist upon there being no potential difference between a part of the system and the earth? Why, there might be a million volts between them!

**Professor Perry.**

The **CHAIRMAN** then called upon **Major Cardew** to reply to the discussion.

**Major CARDEW**: It is not necessary to detain the meeting with much by way of reply. The paper was a very dry one necessarily, and my best thanks are due to the gentlemen who

**Major Cardew.**

Major  
Cardeu.

have enlivened it by the discussion. As regards polarisation, I think there is no doubt there is very considerable polarisation in the body; and from various experiments on the resistance of the human body I have made, I find that as you increase the pressure the apparent resistance steadily diminishes with continuous pressure. I think it tends to go to a minimum. I am inclined to think that when you do get down to the minimum with high pressures, there is not very much difference between the resistance opposed to alternating and continuous currents, although with low pressures there is twice the resistance with continuous than what there is with alternating currents. What Dr. Fleming has said about continuous-current shocks depends very much upon what you are disposed to consider continuous. Of course the current from a Brush machine is capable of giving even Mr. Mordey a shock. But when the current is taken from accumulators a much higher voltage can be taken without distress than from alternating-current machines. In fact, I have known experimenters getting quite enough even with 20 volts alternating current, and asking not to have it carried to any greater extent.

Professor PERRY: Do you mean merely touching the lead?

Major CARDEW: No; I mean through the body, with their hands in salt water, of course. I do not think I can answer Mr. Newman-Lawrence's observations in any way. It is very interesting to know that if you want to pass a shock through your brain there is one way of more effectually doing it. Mr. Addenbrooke, in his remarks, once or twice made use of the expression that the main had 2,500 volts on it, and I understood from his remarks that only one conductor was touched. No doubt there was that voltage in the palmy days of the Grosvenor Gallery; yet when the man touched one of the conductors the voltage was reduced in proportion—probably from  $V$  to  $V_3$ , which comes out just about one-third of  $V$ . This is the diagram [*indicating*]. There are 10,000 ohms insulation on the other main, and 1,000 for the leak, so it is not likely that he got more than one-third; no doubt, however, that was quite enough for him.

Mr. Mordey's remarks show wonderful immunity from shocks,

certainly; but I think the Brush Company's works have got a very good wooden floor—if I remember rightly, a very solid floor, probably treated with——

Mr. MORDEY: It is an ordinary wooden floor, 18 inches above the alluvial soil. If we take a board up, we can see the soil quite damp beneath.

Major CARDEW: Then Professor Ayrton disagrees about the amount of discharge which a man will get through him if he touches one conductor in a perfectly insulated system of large capacity. In the first case I have it connected on to a machine which we may consider unlimited in power as regards sending current. What will be the shock which the man will get? I have said it will be that, if he touches conductor A, he gets a shock equal to the capacity of the other conductor, multiplied by the whole difference of potential. My contention is that the discharge is the sum of two things—first of all, the charge on the conductor touched, which goes to reduce it to the potential of zero; besides that, you have to consider that the other conductor has to be raised to the full potential, and an equal quantity to this increase of charge flows to earth through the body.

The CHAIRMAN: I move that a vote of thanks be accorded to Major Cardew for his paper, which has given rise to a most interesting discussion.

This was carried by acclamation.

A ballot was then taken, at which the following candidate was elected:—

*Associate:*

John Francis Cleverton Snell.

The meeting then adjourned.

## ORIGINAL COMMUNICATIONS.

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### EARTH CURRENTS IN INDIA.

By E. O. WALKER, Member.

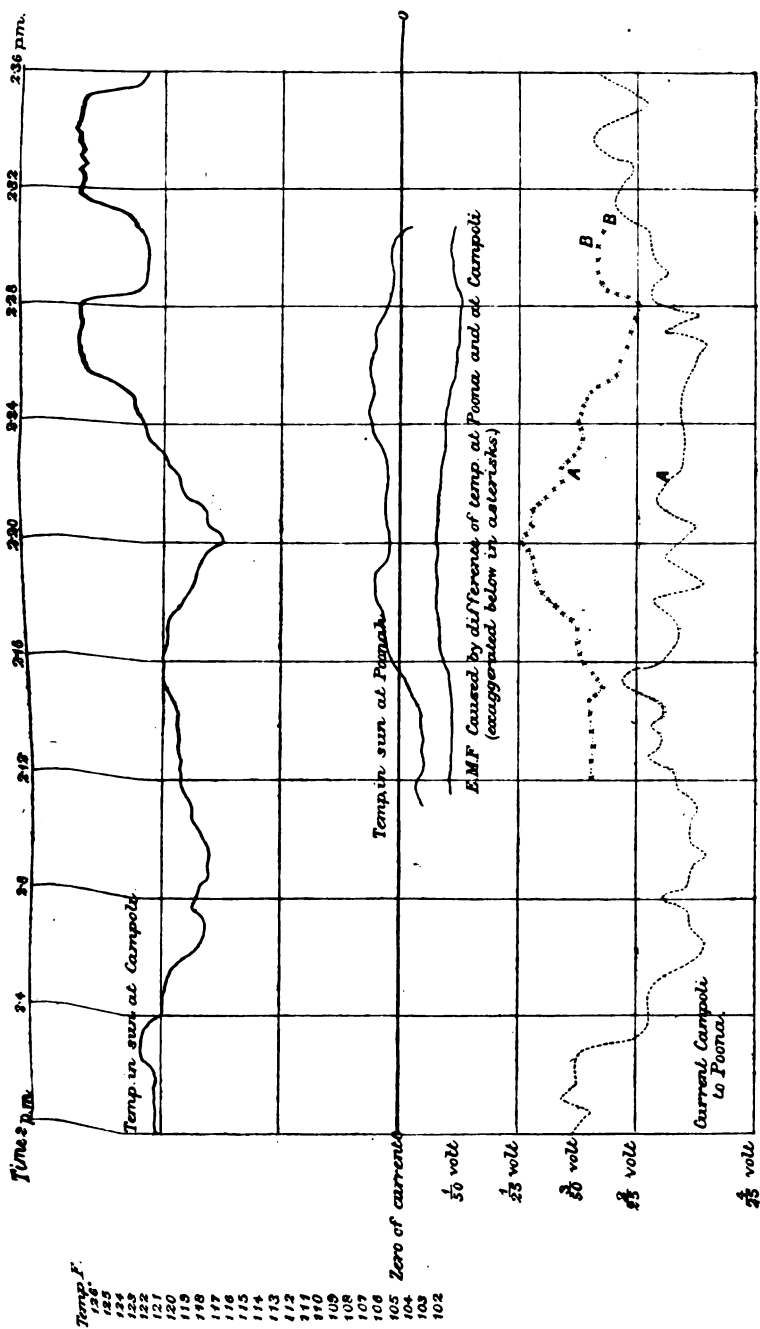
The observations which form the subject of this paper are in continuation of those brought before the Institution of Electrical Engineers on 8th February, 1883, 16th September, 1884, and 31st January, 1888.

An entirely new line of telegraph from Poona to Campoli (on the road to Bombay) afforded a valuable opportunity for observation before polarisation effects should be caused by signalling currents. The line lies mainly east and west, the length being about 47 miles. Poona is situated on the high uplands of Western India, some 1,800 feet above sea level; Campoli at the base of the Western Ghauts as they slope towards the coast, and not more than a few hundred feet above the sea. The difference in temperatures of the two places is therefore very marked, and in the following considerations, will be specially treated. The earth connections at both ends of the line were the cast-iron sockets of the terminal posts and the galvanised iron wire stays. The material of the earth-plates was therefore entirely similar in both cases. Both were distant a mile from the nearest telegraph office, and there was no trace of disturbance by signalling currents upon the galvanometer. The latter was made by Messrs. Elliott Bros. as the Fahie Premium of 1888,\* and is a very sensitive and useful instrument. The suspension fibre is at present coarse, and was attached since receipt of the instrument in India. One volt through 10,000 ohms and the galvanometer (itself 4,000 ohms) deflects the needle 42°. During the observations, the line, earth connections (about 1,000 ohms in all), and galvanometer were alone in circuit.

The diagram containing the results does not itself call for

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\* The Fahie Premium was awarded to Mr. Walker for his paper on Earth Currents, in 1888.—ED.



much explanation. Absolute synchronism in the observations of temperature at Poona and Campoli could not be secured with ordinary timepieces, but the watches used were compared at 8 a.m. and 9 p.m. on the day of observation, and differences allowed for when plotting the curves on the diagram.

What is desired in this paper is to bring prominently to notice the effect, firstly, of the different characters of the soils upon the earth-plates, and thus their influence upon the current; and, secondly, of the variation of the thermal electro-motive force at the junction of the plates and the ground. These effects will be treated separately.

#### ELECTRO-CHEMICAL EFFECT OF SOIL.

The first few readings of the current, from 2 p.m. to 2.3 p.m., showed a current from Poona to Campoli; again at 2.15 and 2.29. This was considered strange, since the currents at this time were generally from west to east, and if produced by a cause invariable from day to day as regards its general features, there should be no such occurrence as a change across the zero line, or, in other words, a change of potential from Campoli to Poona. In view to elucidate the cause of this apparent infringement of the general rule, some soil was collected from the neighbourhood of each terminal post, brought to Poona and packed into a box, the two soils meeting near the centre of the box. The whole was slightly wetted, and two imitation cast-iron posts (both rusty, as were the terminals in the case in point), with galvanised iron wires to represent stays, and galvanised iron caps to represent the upper tubes of the posts, were planted one at each end of the box. There was thus a presentment, in miniature, of the line and earths as they existed during the observations at Poona and Campoli. The caps of the small posts being joined to the terminals of the galvanometer, a deflection of  $21^{\circ}$  was obtained, showing a current from the post buried in the Poona soil through the line to that in the Campoli soil. By calculation this was found to be caused by an E.M.F. of 1-10th volt. The deflection was perfectly steady. In the trace as shown in the diagram, this deflection has been allowed for, and its value separated out. The

current due to solar influence now took its place in the diagram well below the zero line, and during the time of observation flowed, as was anticipated, from Campoli to Poona—*i.e.*, from west to east. Had not the electro-chemical effect of the soils been noted and eliminated, a totally wrong conclusion might have been arrived at as regards the force producing the current, as the latter, under the influence of disturbing causes, changed its direction three times. The electro-chemical effect being eliminated, however, the current preserved one direction, and only fluctuated in strength.

The earth known in India as “black cotton soil,” which was that collected from the neighbourhood of the terminal post at Poona, has been conjectured by some to be the detritus of trap rock, which exists extensively in Western India, as well as by others to be a lacustrine deposit. It has no doubt been deposited by aqueous agency, and in the instance in point has a very different effect upon iron and zinc from that of the grey-green soil surrounding the terminal at Campoli, which has been washed down the face of the igneous rocks composing the mountain ranges of the Western Ghats.

It is very possible that other descriptions of soils would produce more marked disturbances, and that their electro-chemical effect does very often overpower the solar effect. With one end of a line joined to earth at a distance from, and the other in, or near, the sea, there would be considerable preponderance of galvanic action at most times.

#### THERMAL EFFECTS AT JUNCTIONS.

The temperature of the ground in any one locality at one foot below the surface of the soil varies—sometimes several degrees, sometimes scarcely a fraction of a degree—during the 24 hours, and at a depth of three feet there is hardly any perceptible change. I am indebted to Mr. Pedler, Meteorological Reporter to the Government of Bengal, for copies of the “Ground Temperature Observations” at Allahabad and at Calcutta (Alipore) during October, 1887. I have no figures for Poona and Campoli, but the situations of these places make the conditions of relative

temperature somewhat analogous to those of the former places. The mean temperatures for October, 1887, are given below:—

*Alipore.*

	Surface.			One Ft. Deep.			Three Ft. Deep.			Six Ft. Deep.
	5 $\frac{3}{4}$ H.	13 $\frac{3}{4}$ H.	21 $\frac{3}{4}$ H.	5 $\frac{3}{4}$ H.	13 $\frac{3}{4}$ H.	21 $\frac{3}{4}$ H.	5 $\frac{3}{4}$ H.	13 $\frac{3}{4}$ H.	21 $\frac{3}{4}$ H.	13 $\frac{3}{4}$ H.
Mean F.	77·3°	90·2°	79·6°	83·4°	82·9°	83·5°	85·3°	85·2°	85·2°	85·4°

*Allahabad.*

	Surface.			One Ft. Deep.			Three Ft. Deep.			Nine Ft. Deep.
	6 H.	14 H.	22 H.	6 H.	14 H.	22 H.	6 H.	14 H.	22 H.	14 H.
Mean F.	64·9°	79·7°	70·2°	80·6°	79·8°	82·0°	83·4°	83·2°	83·2°	84·4°

There exists, owing to the differences of temperature at all times between ground at these places, a condition favourable for generation of a current from Calcutta (Alipore) to Allahabad, and variations in these differences which lead the current to vary during the 24 hours in strength, but not in direction. But then these temperatures are those taken in the shade. When the junction of one earth-plate and the ground or water is in shade, or partial shade, and that of the other is exposed to the direct rays of the sun, the difference in temperature is so large as to reverse the direction of the thermal current quickly. The points of junction at the surface of the ground or water will, from the "Ground Temperature Observations," readily be understood to be the portions of a telegraphic circuit where thermal effects are most potent, and such was practically proved to be the case by aid of the model circuit referred to above. When a steady current was flowing in the model from Poona to Campoli, the junction of the iron bar and the black cotton soil was heated by the application of lighted alcohol on cotton wool until that portion of the bar rose in temperature to 105° F; the other bar,

representing the Campoli terminal, and the soils in the box had the average temperature of the room, namely,  $74^{\circ}$  F. The temperature of the former was found by applying a small thermometer to the bar after removing the cotton wool. The difference of  $31^{\circ}$  F. in temperature of the two bars at their junctions with the soils increased the deflection given by the current  $4^{\circ}$ . Three experiments were similarly made. In one case the increase was  $7^{\circ}$ . But the conditions of the most satisfactory one were those giving  $4^{\circ}$ . With the particular galvanometer in use, and the resistances then in circuit, an increase of deflection of the needle from  $18^{\circ}$  to  $22^{\circ}$  (as was actually the case) would indicate an access of current of 0.000006 ampere, or the application at the junction of the Poona terminal post with the ground of an E.M.F. of 0.03 volt. This is a gratifying quantitative valuation of the thermal effect of temperature differences ordinarily obtaining during the day-time in India. It is not impossible that there may be differences in the shade and in the sun of  $40^{\circ}$  F., and that any two ends of a line may be subject by fortuitous circumstances to the effects produced by such a difference at any time during the heat of the day. At the time of writing—in Poona itself at 10.30 a.m.—the temperature in the shade is  $82^{\circ}$  F., and in the sun  $115^{\circ}$  F. It is premature to state that 0.03 volt represents the E.M.F. arising from a difference in temperature of  $31^{\circ}$  F. on any part of the thermometrical scale, and whether the character of the soil is not also capable of modifying thermal effects at junctions; but in the experiments under notice the soils were identically the same, both in the model and in the actual case of the line from Poona to Campoli, and the temperatures at these two places, as will be seen from the diagram, varied from  $103^{\circ}$  F. to  $125^{\circ}$  F. The thermal disturbance has therefore been plotted on the diagram from the datum that a difference of  $31^{\circ}$  F. at this part of the thermometrical scale produced an E.M.F. at the hotter of the two junctions in question of 0.03 volt. As will be seen from the curve representing the current, the E.M.F. produced thermally is not by any means large enough to account entirely for the cause of the current. The maximum E.M.F. at any time

producing the current was 0.1 volt, but the maximum thermal force was 0.02 volt. The influence of the thermal effect in modifying the current is well shown in the diagram between the points A to B, where a rise in the difference of temperatures shows an increase in the current from Campoli to Poona, the junctions at Campoli being the hottest; and a fall in the current accompanies a decrease in the difference of temperatures at the two ends of the line. Passing clouds and gusts of wind occasioned marked effects. It was found that the junction of the copper leading wires with the iron line wires had no influence in producing E.M.F., as the temperature of the copper and the iron at their points of union was the same. Speaking generally, it would seem that the same solar cause which produces magnetic variations produces also simultaneous thermal effects. At the same time, it may be well reserved for future experiments to ascertain whether the thermal effects do not account entirely for magnetic phenomena. The E.M.F. produced at the junction of the atmosphere and the earth cannot be disregarded; nor can it be overlooked that a thin superstratum of the earth is subject to daily fluctuations of temperature, while at a depth of a few feet a relatively constant temperature is maintained. So far the maximum E.M.F. observed on the Indian lines occurs in the forenoon, often at 9.30 to 10.30 a.m.; and from the Allahabad-Alipore observations it will be seen that at this period of the day the rise in temperature of the surface soil is rapid; the fall in the afternoon is less so. Mr. Chambers, F.R.S., superintendent of the Colaba Observatory, Bombay, noticed the resemblance of the curve of rise of E.M.F. between Belgaum and Vingorla, which I furnished in the year 1882, with that of the rise of surface temperature in Bombay.

The conclusion to be drawn at present, as regards the observation of earth currents in telegraph lines, is that unless the effects of soils upon the earth-plates, and the thermal effects of differences of the temperature at junctions, are noted and separated out, very misleading generalisations may be arrived at, and that records of currents may be practically worthless. On the other hand, with an unsensitive galvanometer, where the

small currents due to thermal effects have no value, the electro-chemical effect being evaluated, the daily marked features of earth currents may be observed, but their smaller fluctuations escape observation. To ascertain the thermal effect at each moment would be very difficult indeed; and it would seem as if the magnetical observations of our observatories will form the best guides at some future time in arriving at a satisfactory explanation of the phenomena of earth currents, unless, indeed, the effect is purely a thermal one in the earth's strata, when it may be estimated in telegraphic circuits.

BELGAUM, INDIA, 12th March, 1891.

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## ON SIGNALLING ACROSS RIVERS IN INDIA WITH CARDEW'S VIBRATING SOUNDERS.

By W. F. MELHUISE, Member.

Adverting to my paper on "Signalling across Rivers in India," which was read at the Institution of Electrical Engineers on the 10th April, 1890, the following will show how these researches subsequently became of practical value.

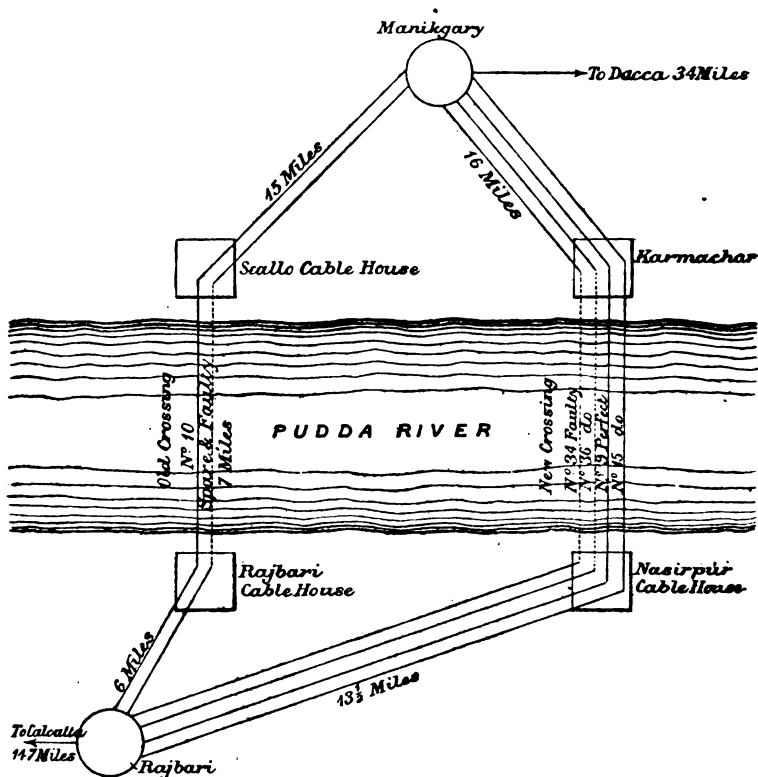
2. Of the several telegraphic circuits which are led out of Calcutta to various parts of India, there are five which branch off almost due east. Three of these circuits convey traffic for Upper and Lower Burma, and the remaining two are utilised for the Dacca, Chittagong, and neighbouring traffic. The country through which the lines above referred to pass, is intersected by rivers, most of which, owing to their great breadth, are cabled.

3. The first river out of Calcutta, called the Pudda, is 7 miles wide, and is cabled at two separate points about 12 miles apart. On the morning of the 8th September, 1890, two out of the five cables which cross this river in the circuits above alluded to, became interrupted. The tests gave evidence of rupture of conductor, with partial earth.

4. The river being in full flood at the time, it was out of the question to attempt to repair the cables; and, moreover, a boat

service to convey messages from bank to bank, though practicable, would have been slow and costly. It was resolved, therefore, to endeavour to utilise the Cardew vibrating sounders.

5. The following sketch shows the length and position of the cables and land lines utilised during the experiments. The dotted line represents a spare cable which has been interrupted for some time owing to an insulated break in the conductor.



6. The following were the experiments tried:—

*Experiment I.*—To work through the guards of the cables at the old, and the guards of the cables at the new crossing, using the former as line and the latter as earth. While this experiment was being carried out the land lines between Seallo and Manickgung got into contact, putting a stop to the experiment. A few signals had, however, been passed through this complete metallic

loop, and from the strength of these signals it is thought the experiment would have been entirely, instead of but partially successful, had the experiment not been interrupted.

*Experiment II.*—To work through the guards of the cables at the east crossing, using any of them as line, with the land line on either side earthed at Dacca and Rajbari. The vibrators were somewhat in the middle of the circuit, *i.e.*, at the two cable-houses.

7. To enable Experiment II. to be made, the Rajbari and Dacca offices were directed to “earth” the connecting land lines, and the vibrators were inserted at the cable-houses at Nasirpur and Kurmachar, the guards of one of the cables being used as line, and the land lines on either side being used as earths. Although this arrangement does not provide a complete metallic circuit for the current, the experiment was nevertheless *quite successful*, the signals being very clear, and readable at a distance of about 4 inches from the ear. The success met with in this experiment is an important and valuable advance on all previous attempts to signal across Indian rivers.

8. In addition to the experiments named, the vibrators were joined up through the severed conductor of the spare cable at the western crossing, and the signals passed through this were also loud and distinct.

9. It will be seen from the foregoing facts that the vibrating sounders have been of practical use in establishing telegraphic communication across one of the widest rivers in India, at a time when no other yet known means might have been possible. The success of Experiment II. greatly made up for the sudden failure in the conductors of two cables, and enabled the Indian Telegraph Department to dispose of a certain amount of local traffic, thereby keeping free the Burma lines for their legitimate work, preventing their congestion, and obviating delays to messages which might otherwise have ensued.

## A B S T R A C T S.

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### **S. TOLVER PRESTON**—THE PROBLEM OF THE BEHAVIOUR OF THE MAGNETIC FIELD ABOUT A REVOLVING MAGNET.

(*Phil. Mag.*, No. 181, Vol. 31, p. 100.)

When a magnet revolves about its magnetic axis, it is a moot point whether its field revolves with it or remains fixed. The author suggests an experiment which, if it can be carried out with sufficient delicacy, would be crucial. A fixed flat disc of metal is placed at one end of a magnet which is thus rotating, and at right angles to its axis. Now, if the *field* is fixed, the magnet will become charged at its poles with positive, and at its equator with negative electricity (or *vice versâ*, according to direction of rotation), due to its revolving in *its own field*. The rotation will have no effect on the plate, for the field which cuts the plate remains at rest; but the plate will be charged on the near side to the magnet with  $-$ , and on the far side with  $+$ . On the other hand, if the field revolves with the magnet, the latter will not be charged; but the plate, being cut by the revolving field, will, by electro-dynamic induction, be charged with electricity, of opposite sign at its centre and periphery respectively. In short, if the field does *not* move, the disc would be polarised by electro-static induction in the direction of its thickness; if the field *does* move, the disc would be polarised by electro-magnetic induction in a direction at right angles to the former. It is suggested that a quartz fibre electrometer might be delicate enough to measure the minute charges which would be produced.

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### **H. NAGAOKA**—MOMENTARY CURRENTS CAUSED BY TWISTING MAGNETISED WIRES.

(*Journal of the College of Science, Imp. Univ. of Japan*, Vol. 3, p. 335, 1890;  
*Beiblätter*, Vol. 15, No. 1, p. 53.)

An iron wire was surrounded by a magnetising coil and fixed between two supports, its ends being joined to a ballistic galvanometer; the supports were so arranged that torsion could be applied to the wire, and the following phenomena, among others, were observed:—If the wire was suddenly twisted when magnetised, or if, while it was twisted, the magnetism was suddenly reversed, a current was induced in it; this current was up to a certain point greater, the greater the magnetising force, reaching a maximum, and then, with further increase of magnetising force, diminishing again: the greater the torsion, the greater the magnetising force at which the maximum occurs. Stretching the wire decreases the momentary current. In a constant field the effect reaches a maximum with increasing torsion, and then diminishes. If the field is increased, the maximum occurs with an increased torsion. Steel behaves exactly like iron, except that the

effect is less strong and the maxima occur with greater magnetising forces. In iron and steel the current is from the south pole to the north when the wire is twisted so as to form a right-handed screw. In nickel the current is in the reverse direction; similar maxima are obtained, but the slope of the curve is less.

# **A. FRANKE—TESTS ON THE WAVE FORM OF TELEGRAPHIC SIGNALS.**

(*Elektrotechnische Zeitschrift*, Vol. 12, p. 103; *Lumière Electrique*, Vol. 39, p. 536.)

This is an account of results obtained with the apparatus previously described in the *El. Zeit.*, vol. xii., p. 6 (cf. *Jour. I. E. E.*, vol. xx., p. 257, April, 1891). Curves are first given of the current, both at the sending and receiving end, of various artificial lines when a single contact is made for  $\frac{1}{2}$  second, followed by a  $\frac{1}{2}$  second break. Resistance, capacity, and inductance are introduced to various extents, and their respective effects clearly shown; and by properly proportioning the two latter, distinctly marked oscillatory charges and discharges are obtained. Similar currents were also sent through 500 km. of looped underground cables; 600 km. of lines which were overhead (iron) except when passing through towns; and also through an equal length of copper telephone line entirely overhead. One point which is strongly brought out by the curves is that in underground cable work the resistance of the battery should be very low, the first momentary rush of current being some nine times greater than the steady value.

The apparatus was then arranged to send the letter "f" through the above three lines at varying rates of speed, and the curves at the receiving end taken. The extreme limit of speed for the overhead copper line is reached at 600 "f's" per minute; at 924 per minute the curve becomes similar to rather indistinct "recorder" strip. In the mixed overhead and cable line, at 150 "f's" per minute the current just drops to the zero line after each dot, and during the dash is oscillatory; at the end of the letter it falls considerably below the zero line. At the same speed, 150, on the cable, the letter is only just legible as "recorder" strip, and quite useless. At this speed, earthing the sending end of the cable through a coil of high inductance (Godfroy shunt) made a marked improvement in the curve at the receiving end.

In the following table  $v$  is the number of "f's" per minute, which, in the three lines, gave practically the same curve at the receiving end;  $R$ , resistance in ohms;  $K$  capacity in microfarads;  $L$ , inductance in secohms:—

LINE.	R.	K.	K R.	$v$ .	K R $v$ .	L R-1.
Cable ... ..	4,800	112	·54	150	81	·0025
Mixed ... ..	8,500	11	·093	800	75	·0014
Copper ... ..	2,600	2	·0052	924	4·8	·0046

It will be seen that in the first two lines K R  $v$  is very nearly the same—i.e., that the speed varies inversely as K R. In the copper line, however, it is not so,

K R  $v$  being only one-sixteenth of what it is in the first two; or, in other words, the speed, calculated from K R, should be about 16,000. The fact is that speed can only be calculated from K R when the time constant, L R<sup>-1</sup>, is small compared to the latter; in this third circuit, as opposed to the other two, it is not so—in fact, it was probably greater, as only the inductance of the instruments at the ends was taken, that of the line being difficult to measure, and comparatively small.

**GRAWINKEL and STRECKER**—THE USE OF SMALL ACCUMULATORS IN PARALLEL WITH DANIELLS FOR HEAVY COUNTRY TELEGRAPH WORK.

(*Elektrotechnische Zeitschrift*, Vol. 12, p. 128.)

This is a sequel to the paper on "Accumulators at the Berlin General Post Office" (cf. *Jour. I. E. E.*, vol. xx., p. 256). For use where there is no opportunity of charging from town mains or private dynamo, the authors have been experimenting on accumulators with plates 6 in.  $\times$  3 in.—two positives and three negatives. The plates are separated by rods, and clamped together by rubber bands; they have an internal resistance of about 0.03  $\omega$  per cell, and will give 1 ampere for twelve hours. Six such cells were experimented on; they were put in parallel with 20 service pattern Daniells (internal resistance 5  $\omega$  each) for 39 days, during which time they received 127 A.H. from the Daniells, and gave out 87 A.H. The authors give the comparative cost of working a station on this system, at present requiring 1,680 Daniells. The latter cost 8½d. each. To replace them would require 30 secondaries at 7s. each, and 100 Daniells. This, allowing a five-years' life for the cells, and the known cost of upkeep for the Daniells, would cost 400 marks per annum, as against 560 marks for the 1,680 Daniells. The six experimental cells are shortly to be used to supply all the local circuits in the Berlin General Post Office.

**E. L. NICHOLS**—ON THE ALTERNATING ARC BETWEEN A BALL AND POINT.

(*Silliman's Journal*, Jan., 1891; *Phil. Mag.*, Feb., 1891, p. 123.)

This is an investigation of the phenomenon that when the two ends of the secondary of a transformer are joined, the one to a ball and the other to a point, and an arc started between them, it is found that a galvanometer in shunt across the arc indicates a considerable flow of direct current through itself. The experiments were made with a small Rhumkorff coil, worked by an alternating dynamo running at 7,000  $\sim$  per min. A steel sewing needle and a brass ball about 1 cm. diameter were used as electrodes, the centre of the ball being in line with the axis of the needle, and the two could be brought together or separated by a micrometer screw. In shunt to the air gap between them was a 2,000  $\omega$  mirror galvanometer, in series with 100,000  $\omega$ . When no spark passes there is no deflection; but as soon as the arc starts there is a steady deflection in one direction, which is reversed if the ball and point are interchanged. At first the arc gives the ordinary note of the

dynamo, the point is a dull red, and the deflection is small and steady. When the point is drawn back, the arc sings louder and more harshly, the point becomes redder, and the deflection larger and very unsteady. When a critical length of arc is reached, the note becomes smooth and even, the point almost white hot, and the deflection much larger and very steady. The explanation suggested is that at first the arc forms both ways nearly alike, and steadily; then that while passing regularly from ball to point it only occasionally passes the other way; finally, it only passes from ball to point. The last effect only occurs very close to the limiting distance at which the arc breaks altogether, and the speed must be kept perfectly steady.

A galvanometer circuit was then arranged so that it was only closed for an exceedingly short time once every revolution of the dynamo, the contact being made by an adjustable brush touching a small contact revolving with the dynamo spindle. Curves of E.M.F. in time in the secondary circuit were taken both with and without the arc, and also of current flowing through the arc. These curves fully bear out the explanation of the phenomenon given above, and show that the striking distance from ball to point exceeds that from point to ball.

The spark was also analysed by a rotating mirror. At first the discharge showed as a succession of equidistant greenish and purple discharges; then, as the distance was increased, the purple discharges were completely suppressed.

Further experiments show that when the spark is passing in both directions, there are two spark paths.

1. The discharge from the ball (positive) leaves the latter in a direction normal to the surface, but enters the other terminal at some distance from its apex.
2. The discharge from the point (positive) leaves the very apex of the latter, but is deflected into a course nearly 45 degrees from the axis, and reaches the ball obliquely at some point on its side. There are grounds for believing, however, that the discharge in this case starts first along the axis, and is gradually displaced as the cycle progresses.

When a thin sheet of platinum was substituted for the brass ball, the spot at which the oblique arc (point +) impinged upon the foil became white hot; that at which the normal arc (ball +) left it, barely red. This agrees with previous observations by Poggendorff and others, that with influence machine or ordinary induction coil spark discharges the negative electrode heats the most, in marked contradistinction to what happens in the ordinary continuous-current arc.

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**Prof. J. J. THOMSON**—ILLUSTRATION OF THE PROPERTIES OF THE ELECTRIC FIELD BY MEANS OF TUBES OF ELECTROSTATIC INDUCTION.

(*Phil. Mag.*, Vol. 31, p. 149, March, 1891.)

The author suggests expressing the processes which occur in the electric field in terms of the changes in form and position of tubes of electrostatic induction which are assumed to be distributed throughout the field. The tubes are all of the same strength; and this strength such, that when a tube falls on a conductor it corresponds to a negative charge on the conductor equal in amount to the charge

which, in electrolysis, we find associated with an atom of a univalent element. The tubes must either form closed circuits, or they must end in atoms; an unclosed tube being one joining two atoms, and the ends of such unclosed tube being places where electrification exists. The ends are on free atoms as distinguished from molecules; the atoms in the molecule being connected by a short tube, whose length is of the order of the molecular distance. Hence the existence of free electricity, whether on a metal, an electrolyte, or a gas, always denotes the existence of free atoms. The production of electrification must be accompanied by chemical dissociation, and *vice versa*. This has long been known to be the case in electrolytes. It has recently been shown to be probably true in gases: gases which conduct readily when hot are those which dissociate when heated; and the passage of electricity through many gases causes chemical changes—e.g., oxygen to ozone. The author then goes on to show that the same may be the case in metallic conductors, although there is, as yet, no such direct evidence as in the case of gases. He suggests that a *true* alloy, after having been traversed by a current, may be found to show signs of electrolysis. The electrical conductivities of metals are, in general, far greater than those of electrolytes, but there is no sharp line of demarcation between them; e.g., silver, mercury, gas-carbon, tellurium, and fused lead chloride have conductivities proportional to 63, 1,  $1 \times 10^{-3}$ ,  $4 \times 10^{-4}$ , and  $2 \times 10^{-4}$  respectively. There is a greater disproportion between the thermal conductivities of silver and cement than between the electrical conductivities of silver and fused lead chloride, but no one argues that heat is therefore propagated by a different method in silver and cement. It is suggestive that substances intermediate in their chemical properties between metals and non-metals—e.g., P, Se, Te—possess conducting properties intermediate between those of metals and electrolytes. Assuming the electro-magnetic theory of light, there is still further evidence. The opacity of thin metal films is enormously less than theory would indicate, if the conductivity of the film for the very rapid electrical vibrations which contribute light were the same as for steady currents. On the view that metallic conduction is really electrolytic, the dissociations and recombinations being extremely rapid, but still taking a finite time,  $T$ , a metal will, to vibrations of a shorter period than  $T$ , act more as an insulator than as a conductor.  $T$  is, in the present way of regarding things, the time taken for a tube of electrostatic induction to disappear, or, rather, to contract to a length comparable with that between the atoms of a molecule, and depends directly on  $K$ , the specific inductive capacity. This, for a good conductor, has never been measured, but it is probably exceedingly large; if, however, it is no greater than that of distilled water which has been measured,  $T$  would be comparable to the time of vibration of a light-wave. The author concludes with a mathematical investigation of the properties of tubes of induction when moving through a dielectric.

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### H. HERTZ—THE MECHANICAL EFFECTS OF ELECTRICAL OSCILLATIONS IN A WIRE.

(*Wiedemann's Annalen*, Vol. 42, p. 407; *Lumière Electrique*, Vol. 39, p. 624.)

Oscillations were set up in the usual way in a loop of wire, as in figure, joined

by a movable bridge,  $a a_1$ . When this latter was at a distance from  $B B_1$  of about 1.2 m., violent oscillation was set up in the upper part, due to resonance between this oscillation itself and the primary oscillation in the air space between  $A A_1$  on the one hand and  $B a a_1, B_1$  on the other. Moving the bridge increases the time of the one oscillation and diminishes that of the other, and thus the best position of the bridge is sharply defined.

**ELECTRO-MECHANICAL EFFECT.**—A small cylinder of gilt paper, 5.5 cm. long and .7 cm. diameter, was suspended horizontally by a single silk fibre; a mirror, and a very small magnet to give directing force, being attached to the cylinder. The whole was then enclosed in a glass box and placed between  $a b$  and  $a_1 b_1$ . When oscillations were set up in the wires, the cylinder tended to set itself in the direction of the lines of electrical force, i.e., in the line joining the two wires. It was first set at  $c$ , the oscillations started, and the amplitude of the first throw of the spot on the scale noted. A series of readings was taken, the slider being moved about 10 cm. further from  $B B_1$  between each two readings. The throws increase from 5.3 scale divisions with the bridge at 80 cm. from  $B B_1$ , to a maximum of 60.6 divisions at 114 cm., and then diminish to 4.2 divisions at 160 cm. A second set of readings was then taken, keeping the bridge fixed, and moving the box along the wire so as to measure the diminution of the effect between  $c$ , the crest of the wave, and the node at  $b$ . The distance  $b c$  was divided into twelve equal parts, and the following readings obtained:—

No.	1	2	3	4	5	6	7	8	9	10	11	12	13
D.	80.5	80.5	79	77	65.6	57.8	50	38.5	27.5	17.5	7	1	0

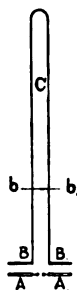
These readings show the actual form of the wave; when plotted, they show a marked divergence from the sine curve. When not suspended between the wires the tube tends to point towards the nearest wire.

**MAGNETO-MECHANICAL EFFECT.**—A ring of No. 14 S.W.G. aluminium wire, 6.5 cm. in diameter, was similarly suspended, and provided with a mirror and directing magnet. Now it might be reasonably expected that this should behave like the cylinder, setting across at the crest of the wave, and being unaffected at the node. This is not the case. When the ring is at the nodal point there is a deflection comparable to that of the cylinder at the crest position; and, further, instead of being "attracted" to the wires, it is, so to speak, repelled, tending to set its horizontal diameter at right angles to the plane of the wires. These phenomena are in themselves sufficient grounds for saying that there is co-existent with the electrical oscillation another one, whose nodes do not agree with those of the electrical one, and whose direction is at right angles to that of the electrical one. Further experiments show that nodes and loops of the one oscillation exactly agree with the loops and nodes of the other. In fact, we are dealing with the magnetic oscillation demanded by theory.

## E. MERCADIER—TELEPHONIC REPRODUCTION OF SPEECH.

(*Comptes Rendus*, Vol. 112, No. 3, p. 156.)

The author considers that the cause of the nasal twang imparted to the voice



when heard through the telephone, is that the fundamental note of the diaphragm and its harmonics are superposed on the note of the voice. To remedy this, he suggests that diaphragms should be used having a higher fundamental note than the highest in ordinary speech (*ut*<sub>4</sub> for the male voice, and *ut*<sub>3</sub> for the female). Two such diaphragms were tried, the one 10 cm. diameter and 1 mm. thick, the other 3 cm. and 0.1 mm.; with both of these the voice was transmitted without any "twang." The weakness with which the letters *l*, *s*, *c*, *u*, &c., are transmitted, and the unnatural loudness of such sounds as *b*, *p*, *r*, *k*, *an*, *on*, &c., is attributed chiefly to the form of the mouth when pronouncing the first class being less favourable to the production of strong sonorous vibrations than when articulating those of the second, but partly also to the above-mentioned natural note of the diaphragm. The fault is much diminished by raising this natural note as before. Another source of indistinctness is the resonance of the body of air behind the diaphragm, which deadens the vibrations set up in the latter. M. Mercadier finds this evil is much diminished by covering the inside of the telephone with felt, and thus reducing the air space.

### **M. MOUREAUX—MAGNETIC VARIATION DURING AN EARTHQUAKE.**

(*Comptes Rendus*, Vol. 112, No. 4, p. 259.)

A violent earthquake occurred on the 15th January on the coast of Algiers, at 4.15 a.m.; one village being almost completely destroyed, and other buildings in the neighbourhood much damaged. The curve of the recording magnetometer at the Parc St. Maur Observatory showed, at 4.15 that morning, a disturbance similar to that which accompanied previous earthquakes at Nice, Gallipoli, and one in Central Asia. The disturbance was 1.5' of arc.

# LIST OF ARTICLES

## RELATING TO

# ELECTRICITY AND MAGNETISM

Appearing in some of the principal Technical Journals during the Months  
of MARCH, APRIL, and MAY, 1891.

S. denotes a series of articles.    I. denotes fully illustrated.

### LIGHT AND POWER.

- F. GERALDY—The Lighting of Paris (S. I.).—*Lum. El.*, vol. 39, p. 7.
- A. DE MONTLAUR—The Lighting of Havre (S. I.).—*Lum. El.*, vol. 40, p. 118.
- F. GERALDY—The Bardon Arc Lamp (I.).—*Lum. El.*, vol. 39, p. 509.
- G. RICHARD—Details of Dynamo Construction (S. I.).—*Lum. El.*, vol. 40, p. 163.
- ANON.—The Meylan-Rechniewski Meter (I.).—*El. Zeit.*, vol. 12, p. 165.
- G. RICHARD—Electric Tramways and Railways (S. I.).—*Lum. El.*, vol. 40, p. 111.
- F. UPPENBORN—The Buda-Pesth Municipal Electric Tramway (I.).—*El. Zeit.*, vol. 12, p. 173.
- BROWN—The Oerlikon High-Tension Transmission Experiments.—*El. Zeit.*, vol. 12, p. 147.
- F. UPPENBORN—Note on the Oerlikon High-Tension Transmission Experiments.—*El. Zeit.*, vol. 12, p. 137.
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# JOURNAL

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The Two Hundred and Twenty-third Ordinary General Meeting of the Institution was held at the Institution of Civil Engineers, 25, Great George Street, Westminster, on Thursday Evening, May 7th, 1891—Professor WILLIAM CROOKES, F.R.S., President, in the Chair.

The Minutes of the Ordinary General Meeting held on April 23rd, 1891, were read and approved.

The names of new candidates for election into the Institution were announced and ordered to be suspended.

The following transfer was announced as having been approved by the Council :—

From the class of Associates to the class of Members—

G. L. Addenbrooke.

Donations to the Library were announced as having been received since the last meeting from the Italian Minister of Posts and Telegraphs; Messrs. Gauthier Villars et Fils; and Mr. Willoughby Smith, Past-President; to whom the thanks of the meeting were duly accorded.

The following paper was then read :—

## ON SOME EFFECTS OF ALTERNATING - CURRENT FLOW IN CIRCUITS HAVING CAPACITY AND SELF-INDUCTION.

By Dr. J. A. FLEMING, Member.

1. The full analytical treatment of the problem of the propagation of an electric current through a conductor possessing capacity and resistance is one which is by no means simple. If to this is added the consideration of the additional condition that both the line and the receiving and transmitting instruments possess self-induction, the problem in its full generality is a very complex one. For certain assumptions as to the mode of variation of the impressed electro-motive force, and with other limitations, it may be said to have been solved for periodic currents; but if we consider the practical case of receiving and transmitting appliances of variable inductance, it can hardly be said that analysis enables the results of definite arrangements to be completely predicted. Fortunately, in much practical work the additional complications which arise from the simultaneous presence of capacity and self-induction in the system are negligible. Useful and existing work is not, however, thus limited, and electrical distribution by means of transformers, alternating currents, and concentric cables forces us to regard phenomena due to the co-existence of inductance, resistance, and capacity in one circuit, which need interpretation, even if only so far as to enable constructive mistakes to be avoided. We have already been made aware by experience that under these circumstances unexpected effects often make their appearance, and these demand examination at this period in the history of the large-scale manufacture and distribution of electric current, to enable us to appreciate the meaning of observed facts. The object of the present paper is to gather together some of the results already arrived at, and to point out a part of the existing state of knowledge of the propagation of alternating currents in circuits in which neither the inductance nor the

capacity are so small that they can be left unconsidered in their effects.

2. It will be convenient to start the inquiry by examining a typical and simple case involving simple periodic electro-motive force, and inductance and capacity in the circuit. Such problems have already been treated analytically by Dr. Hopkinson; geometrically by Mr. Blakesley, Major Cardew, Mr. Kapp, myself, and others.\*

Let A be an alternator feeding an inductive circuit,  $a b f$ , of which the section  $a b$  has inductance  $L$  and resistance  $R$ , and the section  $b f$  has inductance  $l$  and resistance  $r$ .

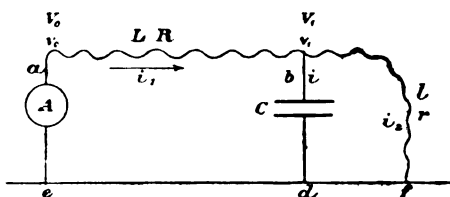


FIG. 1.

At  $b$  let a condenser of capacity  $C$  be inserted, and let the terminals  $e$ ,  $d$ , and  $f$  of the line condenser and alternator be to earth.

Let  $v_0$  be the potential at  $a$  at any instant, and  $v_1$  that at  $b$ ; and let  $V_0$ ,  $V_1$ , be the maximum values of these potentials, supposed to vary harmonically. Let  $i_1$  and  $i_2$  be the currents at any instant in the sections of the line  $ab$ ,  $bf$ , and  $i$  the current flowing into the condenser, and  $I_1$ ,  $I_2$ ,  $I$ , the maximum of the same. Under these circumstances we proceed to investigate what are the relative values of  $V_0$  and  $V_1$ , or their ratio to one another. We have always,

$$i_1 = i + i_2 \quad \dots \quad \dots \quad \dots \quad (1)$$

also, 
$$i = C \frac{dv_1}{dt} \quad \dots \quad \dots \quad \dots \quad (2)$$

and 
$$l \frac{di_2}{dt} + r i_2 = v_1 \quad \dots \quad \dots \quad \dots \quad (3)$$

but we may write, as the currents are simply periodic,

$$i_1 = I_1 \sin p t \quad \dots \quad \dots \quad \dots \quad (4)$$

where  $p = 2 \pi n$ ,  $n$  being the frequency of the periodic current

\* Dr. Hopkinson's paper on "The Theory of Alternating Currents" is in the *Proceedings of the Institution of Electrical Engineers* (late *Journal of the Society of Telegraph-Engineers*) for 1884, vol. xiii., p. 496. Mr. Blakesley's papers are collected in his admirable little book on "Alternating Currents."

in  $a$ ,  $b$ , and  $t$  the time reckoned from the instant when the current in this first section is zero.

By elimination of  $v_1$ ,  $i_1$ , and  $i$  from the above four equations we find easily that

$$C l \frac{d^2 i_2}{dt^2} + C r \frac{d i_2}{dt} + i_2 = I_1 \sin p t \quad \dots \quad (5)$$

Since  $i_2$  must also be a simple periodic current, lagging in phase behind that of the current  $i_1$ , we may take  $i_2$  to be of the form,

$$i_2 = I_2 \sin (p t - \theta) \quad \dots \quad (6)$$

Hence, by differentiation of (6), and substitution in (5), we arrive at

$$(1 - C L p^2) I_2 \sin (p t - \theta) + C r p^2 I_2 \cos (p t - \theta) = I_1 \sin p t \quad (7)$$

which, by a well-known transformation, may be put in the form,

$$I_2 \sqrt{(1 - C l p^2)^2 + C^2 r^2 p^2} \sin (p t - \theta + \phi) = I_1 \sin p t \quad (8)$$

Both sides of this equation (8) are expressions for the same thing—namely, the value of  $i_1$ —hence, equating coefficients, we have,

$$\left(\frac{I_1}{I_2}\right)^2 = (1 - C l p^2)^2 + C^2 r^2 p^2 \quad \dots \quad (9)$$

Putting  $a = 1 - C l p^2$ , and  $b = C r p$ , we have,

$$\left(\frac{I_1}{I_2}\right)^2 = a^2 + b^2 \quad \dots \quad (10)$$

This gives us the ratio of the maximum, or square root of the mean square values, of the currents in the two sections of the line on either side of the condenser.

It is convenient next to shift the origin of the time, and take the instant when the current  $i_2$  is zero as a point from which to reckon the time.

Doing this, we can write,

$$i_2 = I_2 \sin p t \quad \dots \quad (11)$$

and, accordingly, from (10),

$$i_1 = I_2 \sqrt{a^2 + b^2} \sin (p t + \theta);$$

$$\text{or} \quad i_1 = I_2 (a \sin p t + b \cos p t) \quad \dots \quad (12)$$

$$\text{hence,} \quad i_1 - i_2 = I_2 (a - 1 \sin p t + b \cos p t) \quad \dots \quad (13)$$

but from (1) and (2)

$$i_1 - i_2 = C \frac{d v_1}{d t};$$

$$\therefore C \frac{d v_1}{d t} = I_2 (\overline{a - 1} \sin p t + b \cos p t);$$

or 
$$v_1 = \frac{I_2}{C p} (b \sin p t + \overline{1 - a} \cos p t) \quad \dots \quad (14)$$

$$\therefore V_1 = \frac{I_2}{C p} \sqrt{\overline{a - 1}^2 + b^2} = I_2 \sqrt{r_2^2 + p_2^2 l_2} \quad (15)$$

We have next to find the value of  $V_0$ .

$$v_0 - v_1 = L \frac{d i_1}{d t} + R i_1 \quad \dots \quad (16)$$

and if we substitute in (16) the value of  $i_1$  given in (12), and of  $v_1$  given in (14), and integrate, we arrive at

$$v_0 = I_2 \left\{ \left( R a + \frac{b}{C p} - L p b \right) \sin p t + \left( R b + \frac{1 - a}{C p} + L p a \right) \cos p t \right\};$$

but, since  $\frac{b}{C p} = r$ , and  $\frac{1 - a}{C p} = p l$ , we get by proper substitution in the above equation, and by the application of the rule for harmonic maxima, the equation,

$$V_0 = \frac{I_2}{C p} \sqrt{(A^2 + B^2)(a^2 + b^2) + 2 B b - 2 A a + 1} \quad (17)$$

where

$$\begin{aligned} A &= 1 - C L p^2; & a &= 1 - C l p^2; \\ B &= C R p; & b &= C r p. \end{aligned}$$

Equations (15) and (17) give us the values of the maximum or mean potentials at the generator and condenser end of the line  $a b$ . If we divide equation (17) by equation (15), we get a value for the ratio of  $V_0$  to  $V_1$ . A little tedious process of division and substitution brings us finally to the required result, which is,

$$\frac{V_0}{V_1} = \sqrt{(A^2 + B^2) + (1 - C p^2) \frac{R^2 + p^2 L^2}{r^2 + p^2 l^2} + \frac{2 L l p^2}{r^2 + p^2 l^2} + \frac{2 R r}{r^2 + p^2 l^2}} \quad (18)$$

This last equation furnishes us with the required result, viz., the ratio of the maximum or mean potentials at the ends of the inductive line  $a b$  in terms of the coefficients of inductance, capacity, and resistance.

Given  $V_0$ , it enables us to calculate  $V_1$ , and to say whether  $V_1$  is greater than, less than, or equal to  $V_0$ .

3. Let us examine some particular cases. Suppose  $r^2 + p^2 l^2 = \infty$ —that is, suppose the section of the line beyond the condenser removed—then

$$\frac{V_0}{V_1} = \sqrt{A^2 + B^2} = \sqrt{(1 - CLp^2)^2 + C^2 R^2 p^2} \quad (19)$$

This reduced case was treated by Dr. Hopkinson long ago,\* and he showed, as we can easily see, that it is possible for the ratio of  $V_0$  to  $V_1$  to be *less* than unity—that is, for the potential at the receiving end, or at the condenser, to be *greater* than the potential at the sending end.

It is very easy to state the conditions under which this is the case. In order that  $V_1$  shall be greater than  $V_0$ , or that there may be a rise of pressure, we must have

$$\sqrt{(1 - CLp^2)^2 + C^2 R^2 p^2}$$

less than unity; hence we must have

$$(1 - CLp^2)^2 + C^2 R^2 p^2 < 1,$$

$$\text{or} \quad 1 + C^2 L^2 p^4 - 2CLp^2 < 1 - C^2 R^2 p^2,$$

$$\text{or} \quad C < \frac{2L}{R^2 + p^2 L^2} \quad \dots \quad (20)$$

If  $L$ ,  $R$ , and  $p$  are constant, the value of  $C$  which makes the ratio of  $V_1$  to  $V_0$  a maximum is

$$C = \frac{L}{R^2 + p^2 L^2}$$

Hence, if the capacity,  $C$ , of the condenser, measured in farads, is *less than* the quotient obtained by dividing twice the inductance of the line,  $L$ , measured in henries, by the square of the impedance  $(R^2 + p^2 L^2)$ , we shall, under these circumstances, find a rise in pressure at the condenser end. For example, suppose the value of  $n$  or the frequency is 67, then  $p = 421$ , and  $p^2 = 177,241$ . If  $R = 1$  ohm, and  $L = \cdot 1$  henry, then  $R^2 + p^2 L^2 = 1,773$ , and there ought to be a rise in pressure if  $C$  is greater than 112 microfarads; or, if  $R = 1$  ohm, and  $L = 100$  henries, then  $R^2 + p^2 L^2 = 1,772,410,000$ , and there

\* See *Proceedings of the Institution of Electrical Engineers* for 1884, page 513.

ought to be a rise in pressure if  $C$  is greater than one-eighth of a microfarad. Generally speaking, the greater  $L$ , the less will be the critical value of  $C$ . For high frequencies the criterion of rise of pressure at the condenser end is

$$C < \frac{2}{L p^2}.$$

That is to say, the critical capacity is just twice that which will at the given periodicity nullify the inductance of the armature circuit. It is worth while to note that even if the criterion for rise of pressure at the condenser end is fulfilled by  $L$ ,  $R$ ,  $C$ , and  $p$ , yet, as seen from equation (18), a sufficiently small impedance ( $\sqrt{r^2 + p^2 l^2}$ ) in the shunt may nullify the effect and force  $V_1$  to be less than  $V_0$ .

4. These *condenser effects*, as they may be called, are very strikingly shown when currents of high frequency are used. Quite recently, Mr. Nikola Tesla has published some interesting observations and experiments on alternating currents with the very high frequencies of 10,000 to 20,000 alternations per second,\* and amongst these he has described an experiment with a condenser joined across the poles of such an alternator. A machine was used giving about 20,000 alternations per second. Two bare wires about 20 feet in length were attached to the poles, and their ends connected to the terminals of a condenser. The difference of potential between parallel points on these wires was explored by a voltmeter, and it was found that the potential difference of parallel points on the wires steadily rose inch by inch from 65 volts at the terminals of the alternator to 120 volts at the terminals of the condenser; and the writer states that observations on the self-induction of the armature in its position of maximum and minimum inductance showed that the capacity which gave the greatest rise at the condenser end corresponded apparently to that which would about nullify the mean self-induction calculated from the observed maximum and minimum. This entirely corresponds to the above theoretical deduction.

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\* See *Electrician*, March 6, 1891, p. 549.

5. The fact that when a condenser is thus charged by an alternating-current machine its terminal potential difference may be very much greater than that of the machine terminals, appears to have been noticed experimentally prior to the time when the simplest case was mathematically treated by Dr. Hopkinson. In the paper of the latter above referred to, it is mentioned that Dr. Muirhead had observed it experimentally. In the discussion which followed Dr. Hopkinson's paper Mr. Blakesley dealt with the same matter in some geometrical diagrams, and Professor Ayrton mentioned that in 1878 or 1879 Mr. Munro took out a patent for the use of condensers in combination with an alternate-current machine. In any case, the experimental verification of a rise of pressure along the line leading from the machine to the condenser is complete.

6. In his book on "Alternating Currents," p. 55 (2nd edition), Mr. Blakesley has treated by his geometrical methods the case examined above, with the object of showing that the employment of a condenser in this manner in combination with a pair of inductive circuits may under certain conditions cause the current in the remote, or shunt, circuit to be greater than the current in the circuit in connection with the machine; and he shows there that a similar criterion to the one given above applies also to the relative current-strengths in the two sections—viz., that the current  $I_2$  in section  $b f$  will be greater than the current  $I_1$  in section  $a b$  if  $C$  is less than  $\frac{2 l}{r^2 + p^2 l^2}$ .

Hence, taking the two separate cases of a simple condenser and a shunted condenser, we have that the maximum or mean potential  $V_1$  at the terminals of the condenser will be less than, equal to, or greater than the maximum or mean potential at the terminals of the machine  $V_0$ , according as the capacity,  $C$ , of the condenser is greater than, equal to, or less than the quantity,  $\frac{2 L}{R^2 + p^2 L^2}$ , in numerical value; and also that the maximum or average current-strength in the remote, or shunt, section of the line will be less than, equal to, or greater than the maximum or mean current-

strength in the section of the line near to the machine, according as the capacity,  $C$ , of the condenser is greater than, equal to, or less than the quantity,  $\frac{2l}{r^2 + p^2 l^2}$ , in numerical value. Note that if  $R$  is measured in ohms and  $L$  in henries,  $C$  must be measured in farads.

7. If we look again at the equation,

$$\left(\frac{V_0}{V_1}\right)^2 = (1 - CLp^2) + C^2 R^2 p^2,$$

and note what are the conditions under which  $V_1$  will be *greater* than  $V_0$ , or the pressure at the condenser end greater than the pressure at the machine end of the line, we see that it depends upon whether the right-hand expression, when worked out numerically, is a proper fraction. Since almost any capacity which occurs in practice is numerically represented by a very small fraction when the farad is the unit, if  $p^2$  is not very great  $C^2 R^2 p^2$  will nearly always in practice be a proper fraction when we insert values of  $C$  and  $R$  in farads and ohms. Hence the condition of rise of pressure will depend almost always on whether  $CLp^2$  is numerically greater than 2 when  $C$  and  $L$  are measured in farads and henries; and if  $C$  is very small, either  $L$  or  $p$  must be very large to secure this result, and *vice versa*. Hence, generally, if the inductance of the machine circuit is very large, a comparatively small capacity across its poles may give rise to a very great condenser effect; but if the inductance of the machine is small, a very large capacity will be needed to bring about the increased pressure at the terminals of the condenser.

Suppose, for instance, that a transformer is used to raise the pressure of an alternator, and that across the high-pressure terminals of the transformer is placed a condenser of capacity  $C$ . This may be either a condenser of the ordinary form, or a certain length of concentric cable insulated at the far end and having a definite capacity. Let us suppose that the high-pressure circuit of the transformer has a great many turns of wire. The mean inductance of this high-pressure coil is then the  $L$  in our formula, its resistance is our  $R$ , and  $p^2$  is  $4\pi^2 n^2$ , where  $n$  is the frequency of the alternator. To fix our ideas, let  $n = 83 = 10,000$  alternations per minute, then  $p^2 = 273,947$ .

Let  $R = 10$  ohms, and  $L = 200$  henries—values possible to be reached in practice—then, if  $C$  is measured in microfarads, we see that that  $(1 - CLp^2)^2 + C^2 R^2 p^2$  becomes

$$\left(1 - \frac{C \times 273,947 \times 200}{10^6}\right)^2 + \frac{C^2 27,394,700}{10^{12}} = (1 - 55C)^2 + \cdot 00003 C,$$

nearly.

If  $C$  is absolutely zero, the value of this quantity is unity. If  $C$  is equal to 1-27th of a microfarad, this quantity is also, roughly, equal to unity. If  $C$  has any value between zero and 1-27th of a microfarad—say 1-50th of a microfarad—then, since  $\left(1 - \frac{55}{50}\right)^2 + \cdot 00003 \frac{1}{50} = \cdot 01$ , nearly, we see that the ratio of  $V_0^2$  to  $V_1^2$  will be 1 to 100, or  $V_1$  will be ten times  $V_0$ . Hence it is obvious there is a narrow range of capacity within which the pressure at the condenser terminals may be enormously increased over that at the machine terminals; but if the capacity is altered so as to take it outside these critical limits, the so-called resonance effects will vanish.

The best way to see how the value of  $V_1$ , or the condenser potential, varies with variation of the capacity, other quantities being constant, is to plot the equation,

$$V_1 = \frac{V_0}{\sqrt{(1 - CLp^2)^2 + C^2 R^2 p^2}},$$

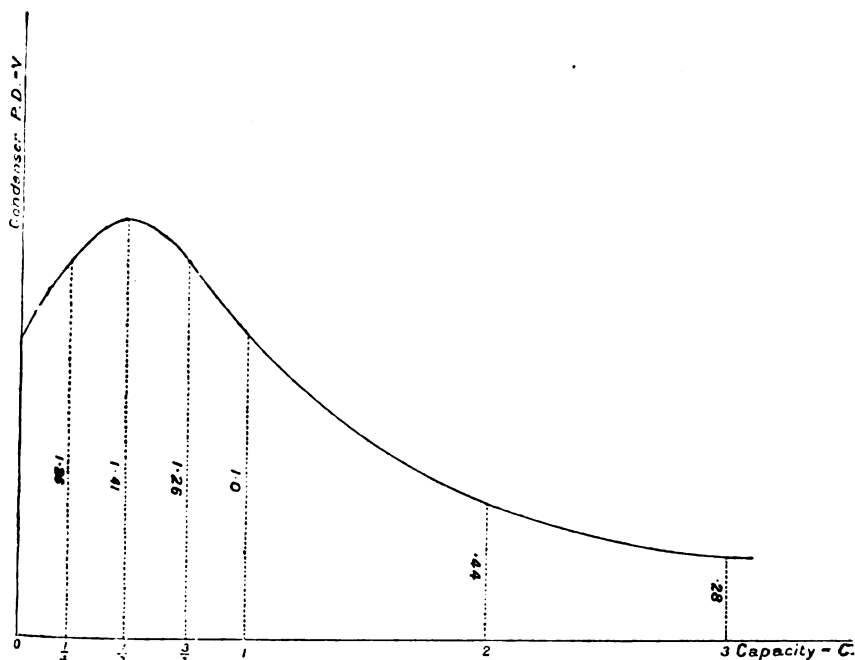
or—which comes to the same thing, and is simpler to plot—the equation,

$$y = \frac{1}{\sqrt{(1 - x)^2 + x^2}}.$$

This has been done in Fig. 2, and a curve representing the variation of  $V_1$  in terms of  $C$ , or of the condenser plate P.D. in terms of its capacity, supposed variable, is there drawn, on the assumption that  $V_0$ ,  $L$ ,  $R$ , and  $p^2$  have certain constant values. We see that the curve ordinate has a maximum corresponding to a certain capacity, and that beyond that point increase in capacity continuously diminishes the condenser plate P.D.

The condenser effect will be at a maximum when  $C$  and  $L$

have such magnitudes that  $CLp^2$  is unity, and  $C$  has at the same time a small value. In this case  $V_1$  may be hundreds or thousands of times greater than  $V_0$ . In testing condensers with alternating-current machines, it is very important not to have that particular critical relation between  $C$ ,  $p$ , and  $L$ , or the



Curve plotted from the Equation,  $y = \frac{1}{\sqrt{(1-x)^2 + x^2}}$ ,

$$\text{or } V_1 = \frac{V_0}{\sqrt{(1 - CLp^2)^2 + C^2 R^2 p^2}}$$

FIG. 2.

capacity, frequency, and inductance of the machine, which will bring about the resonance effect, or else the testing process may be a destructive process as far as the condenser is concerned.

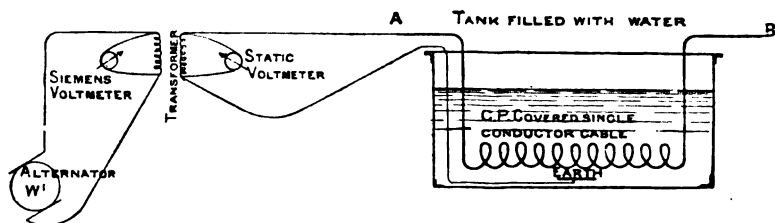
8. Quite similarly, we can show that if the capacity of the condenser is fixed, there is a certain range of inductance of the circuit of the alternator within which the resonance effect takes place. For example, let  $C = .1$  microfarad, as before,  $p^2 =$

273,947, and  $R = 10$  ohms, then for the ratio of  $V_0^2$  to  $V_1^2$  we have the value,

$$\left(1 - \frac{273,947 L}{10^7}\right)^2 + \frac{273,947 \times 100}{10^8} = (1 - .027 L)^2 + .27.$$

If  $L = 0$ , this ratio is greater than unity; if  $L = 74$ , the ratio is again greater than unity; but for values of  $L$  between 0 and 74, the ratio is less than unity. In other words, within these limits of the inductance the pressure at the poles of the condenser is raised.

The above theory is shown to be confirmed in its general character by the results of some highly interesting experiments, made more than a year ago (April 10, 1890) at the works of Messrs. Siemens Bros., which Mr. Alexander Siemens kindly sent to me. A Siemens  $W_1$  alternator was kept running at a constant speed of 750 revolutions, and the exciting current was also kept constant. The current from this was sent into the thick-wire coil of a No. 2 Siemens transformer. The resistance of the primary circuit of this transformer is 0.115 ohm, and that of the secondary circuit 129 ohms. The terminals of the fine-wire circuit were connected to various lengths of single-conductor cable, having gutta-percha insulation and immersed in a tank; the cables being insulated at the far ends, and forming thus a condenser attached to the secondary of the transformer (see Fig. 3).



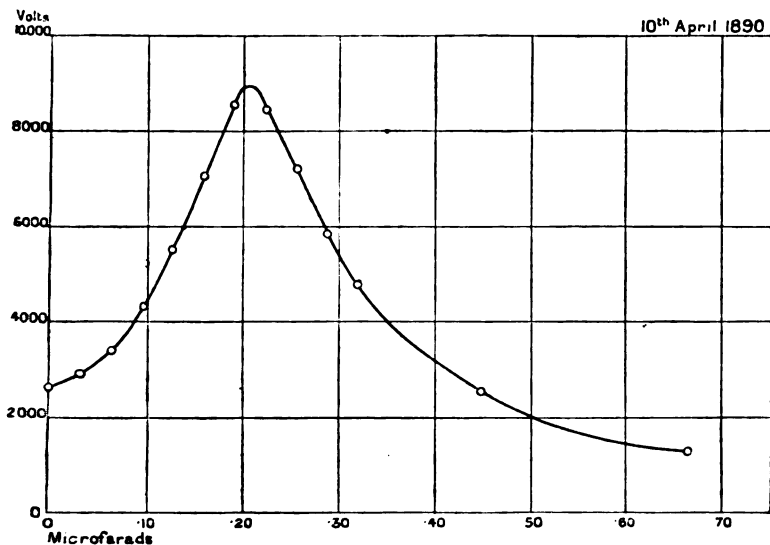
The Arrangement of Alternator, Transformer, and Cable in the Experiments on Condenser Effects made at Messrs. Siemens's Works, April, 1890.

FIG. 3.

The pressure was measured at the terminals of the machine, and at the terminals of the transformer on the secondary, or condenser, side. The experiments consisted in varying the length of the cable used so as to vary the capacity, and in observing

the pressure at the two sides of the transformer whilst the dynamo was kept running at constant speed and excitation. The results are embodied in the following series of interesting curves. The horizontal distances in the diagrams represent the various capacities of the open-circuit cables or condenser attached to the terminals of the secondary of the transformer. The vertical ordinates in Fig. 4 represent the observed P.D. of the two surfaces of this condenser—that is, the P.D. at the secondary terminals of the transformer.

It will be seen from Fig. 4 that when the capacity is zero this P.D. was 2,500 volts. As the length of cable was increased continuously, this P.D. rose up to 8,500 volts, corresponding to a

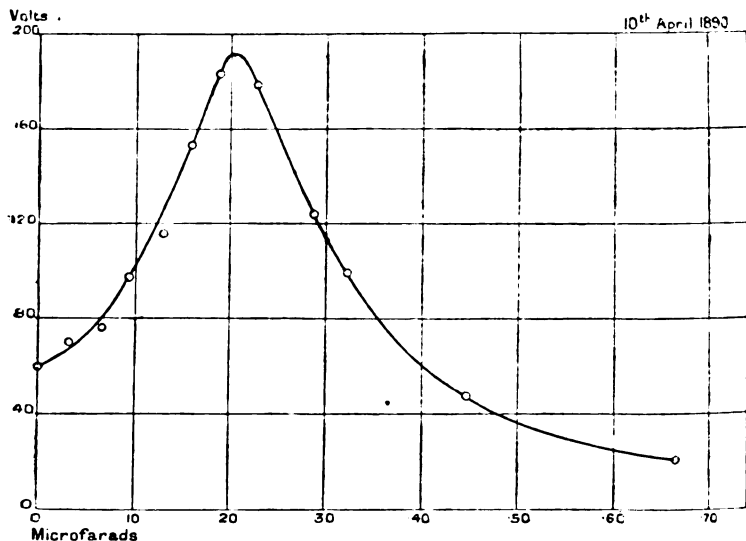


Curves showing the Variation in Voltage in the Secondary (high voltage) Circuit of a Transformer by Varying the Capacity in the Secondary (high voltage) Circuit. The experiments were made with a No. 2 Siemens transformer, and current was obtained from a W<sup>1</sup> Siemens machine, frequency being 100  $\sim$ . The exciting current was kept constant, and the alternator was kept running at 750 revolutions during all the experiments.

FIG. 4.

capacity of .2 microfarad, and then fell again to 2,500 volts, at a capacity of .45 microfarad. Hence, corresponding to a certain capacity—viz., .2 microfarad—the pressure is multiplied about  $3\frac{1}{2}$  times, and there is a certain narrow range of capacity over which

this exaltation of pressure takes place. At and beyond .45 microfarad capacity, the presence of the condenser reduces the normal pressure continuously. The pressure on the primary side of the transformer is also increased, as shown in the curve given in Fig. 5, which represents a series of similar pressure observations

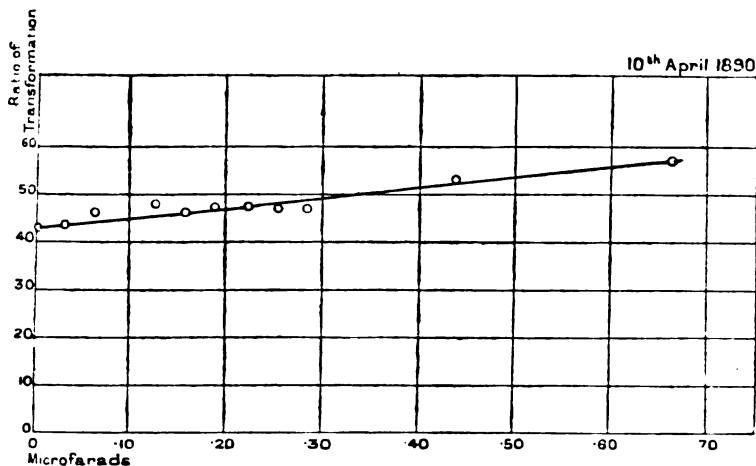


Curves showing the Variation in Voltage in the Primary (low voltage) Circuit of the same Transformer by Varying the Capacity in the Secondary (high voltage) Circuit. The experiments were made with a No. 2 transformer, and current was obtained from a W<sup>1</sup> Siemens machine, the frequency being 100  $\sim$ . The exciting current was kept constant, and the alternator was kept running at 750 revolutions during all the experiments.

FIG. 5.

taken on the primary side of the transformer. It will be seen that the same general effect takes place on the primary side. It will also be seen how well these curves deduced from observation agree as regards general form with the theory as illustrated by the curve in Fig. 2. The curves in Figs. 4 and 5 are generally similar, but, if superimposed, will be found to be not quite so. If the ratio of corresponding ordinates is set off so as to form a fresh curve (Fig. 6), this curve will give the variation of change ratio of the transformer corresponding to the various capacities, and we see that this change ratio is altered progressively from 1 : 43 to 1 : 57, as the capacity of the condenser on the secondary is

changed from zero to .65 microfarad. It is evident, therefore, that in applying transformers to test cable, such as concentric



Curves showing the Dependence of the Ratio of Transformation of Pressure from Primary to Secondary Circuit on Capacity of Secondary, resulting from Figs. 4 and 5.

FIG. 6.

cable, which possesses sensible capacity, we must be on our guard against this alteration of the change ratio of the transformer, or else we may be misled as to the real pressure being applied to the cable.

9. These considerations point out that in applying either transformers or alternators to circuits which have capacity, we must have regard to the fact that for certain critical values of the inductance and the capacity for given frequencies, there may be a very considerable increase in the potential difference of the two sides of the condenser over and above that of the poles of the alternator or transformer when no condenser is present. Such effects have been noticed in practice of late. It has been frequently found that if a transformer which gives on its open secondary circuit a certain P.D. is applied to test the insulation of a length of concentric cable by connecting its terminals to the two conductors, the far ends of the cable being insulated, under some circumstances an increased P.D. is observed between the free ends of the cable over and above that at the terminals of the transformer when the cable is not connected to it. When trans-

formers have been employed to test the insulation of armatures of high-pressure alternators, it has been noticed that under some circumstances large snapping sparks will jump off over air distances which could not possibly permit discharges at the actual P.D. of the secondary terminals of the transformer so used when unconnected with any such body having capacity.

In employing an alternator or transformer to test insulation between two opposed conductors, or in any way in which the opposed terminals gain capacity, it is necessary to be on the watch for this possible rise of pressure, and not to infer, because a transformer gives, say, 2,000 volts on open circuit between its poles when measured with an electrostatic voltmeter, that therefore this is the measure of the pressure which is being applied to a dielectric which forms the insulator of a condenser joined in across the poles of that transformer.

I am not at all sure that under some circumstances errors may not be committed in the mere use of an electrostatic voltmeter to measure P.D. between the poles of an alternator or a transformer, if that voltmeter happens to possess a certain capacity in relation to the inductance of the circuit in which it is joined in parallel.

10. It is easy to treat in the foregoing manner another case slightly more complicated.

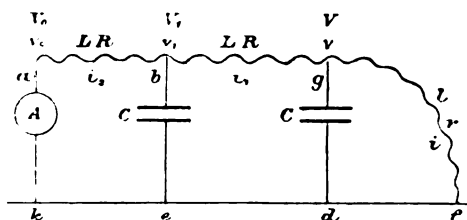


FIG. 7.

Let the inductive line  $a b g f$  (Fig. 7) be divided into three sections,  $a b$  and  $b g$  having each resistance  $R$  and inductance  $L$ , and the section  $g f$  having  $r$  and  $l$  for similar quantities. Let condensers of capacity  $C$  be inserted in  $b$  and  $g$ . If  $v$  and  $v_1$  are the potentials at any instant of the condenser terminals, and  $i$ ,  $i_1$ , and  $i_2$  the instantaneous currents in the sections of the line,

and  $I_1$ , and  $I_2$  the maximum values of these periodic currents, it is not difficult to show, with our former notation, that

$$v_1 = \frac{I}{Cp} \left\{ (B a + A b) \sin p t + (B b - A a + 1) \cos p t \right\} \quad (21)$$

but 
$$i_2 = i_1 + C \frac{d v_1}{d t},$$

and 
$$i_1 = I (a \sin p t + b \cos p t) \quad \dots \quad (22)$$

by (12). Hence we have,

$$i_2 = I \{ (A a - B b + a - 1) \sin p t + (B a + A b + b) \cos p t \} \quad (23)$$

from which we deduce that the ratio of the maximum currents,  $I_2$  and  $I_1$ , in the first and second sections of the line is given by the equation,

$$I_2 = I \sqrt{(A + 1^2 + B^2)(a^2 + b^2) + a b A - B - 2 a A + 1 + 2 B b + 1} \quad (24)$$

and in like manner the ratio of the maximum values of the potentials  $V_0$  and  $V_1$  can be obtained.

It is possible also to show that for certain critical values of  $L$ ,  $C$ , and  $R$ , the potentials  $V_0$ ,  $V_1$ , and  $V$ , at the terminals of the alternator and at those of the condensers, progressively increase, as well as the currents in the sections of the line. Cases of this kind, beyond a single condenser, are best treated as Mr. Blakesley has done—by geometrical methods.

11. In the limited case of a condenser charged by an alternating electro-motive force through an inductive circuit, we may arrive at a direct analytical solution of the relation between the coefficients of induction and capacity of the system and the mean square of the charging current, and of the potential difference at the poles of the condenser or at those of the alternator, as follows:—As before, let  $V_0$  and  $v_0$  stand for the maximum and instantaneous values of the potential difference at the poles of the alternator; let  $V_1$  and  $v_1$  be the same quantities at the terminals of the condenser; and let  $I$  and  $i$  be the same for the current flowing into the condenser having a capacity  $C$ , whilst the line connecting the condenser with the alternator has an inductance  $L$  and a resistance  $R$ . We have then, by fundamental equations, the following relations:—

$$L \frac{d i}{d t} + R i = v_0 - v_1 \quad \dots \quad \dots \quad (25)$$

$$i = C \frac{d v_1}{d t} \quad \dots \quad \dots \quad \dots \quad (26)$$

Also, let  $v_0 = V_0 \sin p t$ , and hence

$$i = I \sin (p t + \theta),$$

where  $\theta$  is the angle of lag of current behind the impressed electro-motive force.

Eliminating  $v_1$  from the above equations, we have,

$$L \frac{d^2 i}{d t^2} + R \frac{d i}{d t} + \frac{i}{C} = p V_0 \cos p t \quad \dots \quad (27)$$

or since, for simple periodic currents,  $-p^2 = \frac{d^2}{d t^2}$ , we have,

$$\frac{d i}{d t} + \frac{1 - C L p^2}{C R} i = \frac{p V_0}{R} \cos p t \quad \dots \quad (28)$$

This equation is of the form,

$$\frac{d i}{d t} + P i = Q \cos p t,$$

the solution of which is known to be

$$i = \frac{Q P}{P^2 + p^2} \cos p t + \frac{Q p}{P^2 + p^2} \sin p t;$$

and an easy transformation gives us, therefore, the solution for the particular case in hand as

$$i = \frac{(1 - C L p^2) \cos p t + C R p \sin p t}{(1 - C L p^2)^2 + C^2 R^2 p^2} C p V_0 \quad (29)$$

This can be written also,

$$i = \frac{C p V_0}{\sqrt{(1 - C L p^2)^2 + C^2 R^2 p^2}} \sin (p t - \phi) \quad (30)$$

and the maximum value of  $i$ , viz.,  $I$ , is, therefore,

$$I = \frac{C p}{\sqrt{(1 - C L p^2)^2 + C^2 R^2 p^2}} V_0 \quad \dots \quad (31)$$

This gives us the mean square, or dynamometer, value of the current  $I$  flowing into the condenser in terms of the mean square value of the potential difference of the poles of the alternator and

the coefficients  $C$ ,  $L$ ,  $R$ , and  $p$ . If  $C L p^2 = 1$ , then the value of  $I$  reduces to  $\frac{V_0}{R}$ ; in other words, for this particular value of the relation between the inductance and the capacity the whole system of inductive charging line and condenser acts as if it were a non-inductive conductor of resistance  $R$ .

We see also that this equation (31) can be made use of to determine  $L$  from observed values of  $I_1$ ,  $V_0$ ,  $C$ ,  $R$ , and  $p$ , or else  $C$  when  $L$  is known. The value of the mean inductance of the line is given by the equation,

$$L = \left( \frac{1}{C p^2} - \frac{1}{p} \sqrt{\frac{V_0^2}{I^2} - R^2} \right) \quad \dots \quad (32)$$

which gives  $L$  in terms of observable quantities.

The current at any instant flowing across the dielectric of the cable may be spoken of as the *condenser current*. It is always in magnitude equal to the product of the capacity of the condenser and the time rate of change of the mean potential difference,  $v_1$ , of its surfaces. It follows that, in the case of simple periodic currents, the square root of the mean square of this current,  $I$ , which is what we measure with a dynamometer, is equal to the product of the square root of the mean square of the condenser plate potential difference,  $V_1$ , and the quantity  $C p$ , where  $C$  is the capacity of the condenser, and  $p$ , as usual, is  $2 \pi$  times the frequency, or  $I = C p V_1$ . This follows at once from equations (19) and (31). The value of the potential difference at the terminals of a condenser of a given capacity, as measured, say, by an electrostatic voltmeter, is therefore determined by the magnitude of the condenser current flowing into it, as measured by a dynamometer when the frequency of the current is known.

12. The writer witnessed some experiments made in the Standardising Laboratory by Major Cardew, in which measurements of the above kind were carried out. A condenser stated to have a capacity of .5 microfarad, as determined by the usual ballistic galvanometer method, was joined across the poles of an alternator, a Thomson deciampere balance was put in the charging circuit, and a Thomson multicellular electrostatic voltmeter across the terminals of the condenser. The readings of

the voltmeter,  $V_1$ , and of the ampere balance,  $I$ , were taken simultaneously, with the following results :—

Frequency of Current, $n$ .	Volts across Condenser Poles, $V_1$ .	Charging Current in Amperes, $I$ .	
		Observed.	Calculated.
103	360	·132	·116
103	396	·145	·128
103	493	·175	·160
103	583	·200	·189
90	481	·150	·136
90	598	·187	·170

Corresponding to a frequency  $n = 103$ , we have  $p = 2\pi n = 647$ , and for  $n = 90$ ,  $p = 565$ .

Hence, since the condenser capacity,  $C$ , = ·5 microfarad =  $5 \times 10^{-7}$  farads, we have as the values of  $Cp$  for the respective speeds  $3,235 \times 10^{-7}$  and  $2,825 \times 10^{-7}$ , and the values of the condenser current are calculated from the relation  $I = CpV_1$ . The calculated values of the currents are all seen to be lower than the observed, and in about the same ratio, viz., about 10 per cent. If, on the other hand, we employ the observed values of the condenser currents and potentials to calculate the condenser capacity, the result is a value greater by about 10 per cent. than the value of the capacity as determined by the ballistic galvanometer. Further experiments showed better agreement in one condenser, but the cause for this difference requires further investigation.\*

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\* In these experiments it was considered at first that the cause of the difference between the condenser capacity, as determined by the alternating current at a high pressure, and what may be called the low-pressure capacity value, as determined by a ballistic galvanometer method, was to be looked for in an error in the frequency; but subsequent experiments, in which the frequency was determined with great care, showed that this was not the case. Again, this 10 per cent. priority of the alternate-current high-pressure capacity over the direct-current

13. The relations between the capacity and self-induction necessary to bring about an increase of pressure at the condenser terminals when an alternator charges a condenser are well displayed by the use of a clock-diagram of electro-motive forces. Such diagrams have been given by Mr. Kapp in some recent papers, and the particular one here given is practically the same as one given to me by Major Cardew, who has also examined this particular problem. The following mode of regarding it will be found to be an easy one. Construct a clock-diagram of electro-motive forces (see Fig. 8) for the simple case of a condenser charged through an inductive line by a constant harmonic impressed electro-motive force.

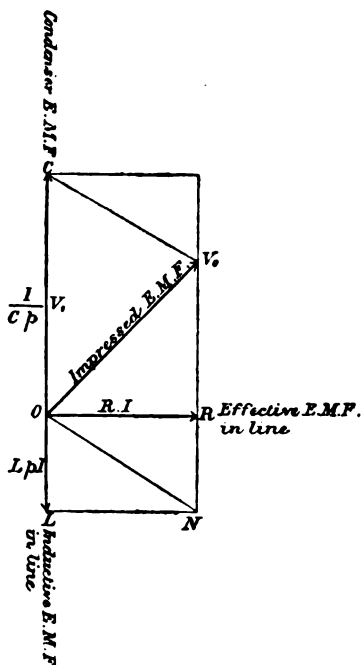


FIG. 8.

O, in Fig. 8 represent the maximum values of various electro-motive forces. Let  $O V_0$  be the maximum value of the impressed E.M.F. at the dynamo terminals, and  $O C$  that at the condenser terminals;  $I$  the maximum current in the line. Take any line  $O R$  to represent the effective E.M.F. in the line, and therefore equal to the product  $R I$ , where  $I$  is the current charging the condenser, and  $R$  is the resistance of the line. From  $R$  draw  $R L$  at right angles to  $O R$ , and let  $R L$  be set off equal to  $L p I$ ;  $L$  being the inductance of the line, and  $p$

low-pressure capacity seemed to exist in two other mica condensers tried. In a good paraffin paper condenser the difference was found to be only 5 per cent. It is not by any means obvious how far we ought to expect to find agreement between the numerical values of the capacity of a condenser determined in these two different ways, particularly when the dielectric is capable of absorption, of conductivity, and of potential electrolysis. It will be very much a question of how the steady capacity was determined, and at what pressure.

being, as usual,  $2\pi$  times the frequency of the current. Note that when a quantity varies harmonically the product of  $p$  and its maximum value gives us the maximum value of its time rate of change, or time differential, whilst the quotient of its maximum value by  $p$  gives us the maximum value of its time integral. Accordingly,  $O L$ , which is made equal to  $L p I$ , represents the counter electro-motive force of self-induction, or back E.M.F., in the line as far as is due to its own inductance, and is in quadrature with  $R I$  as regards phase. Hence the resultant of these two is  $O N$ . Next, set off  $O C$  at right angles to  $O R$ , and in advance of it, and make  $O C$  equal to  $\frac{I}{C p}$ , that is, to the maximum value of the quotient of the time integral of the charging current by the capacity of the condenser. This gives us the counter electro-motive force of the condenser, or the E.M.F. at the terminals of the condenser,  $V_1$ . We have then to take the resultant of this last and  $O N$ , which gives us  $O V_0$ , and this last resultant is the impressed electro-motive force at the dynamo terminals.

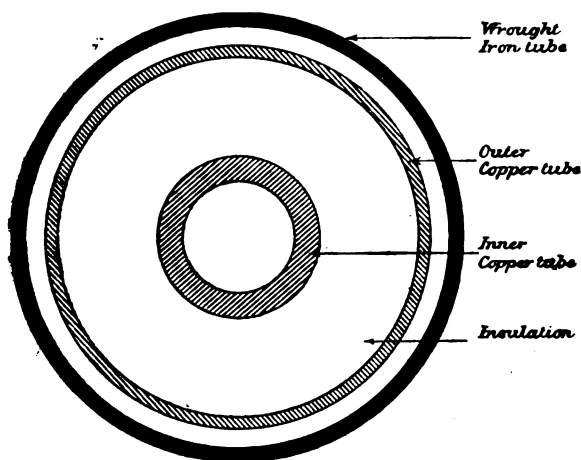
It is necessary to bear in mind that the counter electro-motive force,  $O C$ , at the terminals of the condenser is in quadrature with the current flowing into it, and in advance of it in phase. The diagram shows us at once that the pressure,  $V_0 = O V_0$ , at the terminals of the dynamo may be less than, equal to, or greater than the pressure,  $V_1 = O C$ , at the terminals of the condenser; and that this is determined by whether the length of the line,  $O V_0 = \sqrt{O R^2 + V_0 R^2}$ , is greater or less than  $O C$ . We see that  $V_0 R = O C - O L = \frac{I}{C p} - L p I$ , and hence that  $O V_0$  is equal to the square root of  $\left\{ R^2 + \left( \frac{1}{C p} - L p \right)^2 \right\} I^2$ . Hence the maximum value of the charging current,  $I$ , is given by the value of

$$\frac{C p V_0}{\sqrt{R^2 C^2 p^2 + (1 - C L p^2)^2}}.$$

Also, we have the same quantity given by the relation  $I = C p V_1$ ; hence the ratio of  $V_0$  to  $V_1$  is given by the value of the quantity  $\sqrt{(1 - C L p^2)^2 + C^2 R^2 p^2}$ , as we have already seen analytically.

The clock-diagram has the advantage that it shows us at once the various lags, and gives explicitly to the eye a proof that a gradual increase in the value of the inductance,  $L$ , from zero upwards is accompanied by a progressive decrease in the value of the terminal pressure,  $V_0$ , at the dynamo, as compared with the terminal pressure,  $V_1$ , at the condenser.

14. In the next place, I wish to place before you an analysis of some experiments which have been made on great lengths of the Ferranti trunk mains between London and Deptford, and which illustrate these principles in a remarkable manner. We are all aware that these cables are concentric cables, and that they possess, accordingly, very measurable electrostatic capacity. The following are the data of the dimensions of these cables, kindly given to me by Mr. Ferranti. The inner conductor, or inner member, is a copper tube (see Fig. 9) of which the inside



Section of Ferranti Trunk Main—Full size.

FIG. 9.

diameter of cross section will be called  $D_1$ , and radius  $R_1$ , and the outside diameter  $D_2$ , and radius  $R_2$ . The outer conductor, or member, is a copper tube of which the inside diameter will be called  $D_3$ , and radius  $R_3$ , and its outside diameter  $D_4$ , and radius  $R_4$ . The insulation between the inner and outer members is compressed brown paper and black wax. The

values of the diameters and radii of cross section of these tubes are as follows (see Fig. 7):—

$$\begin{aligned} D_1 &= \frac{9}{16} \text{ of an inch} = \cdot 5625 \text{ in.} \\ D_2 &= \frac{13}{16} \text{ ,, ,,} = \cdot 8125 \text{ ,,} \\ D_3 &= 1\frac{27}{32} \text{ ,, ,,} = 1\cdot 8437 \text{ ,,} \\ D_4 &= 1\frac{15}{16} \text{ ,, ,,} = 1\cdot 9375 \text{ ,,} \end{aligned}$$

Hence we have—

$$\begin{aligned} R_1 &= \cdot 281 \text{ in.} & R_1^2 &= \cdot 0789 \\ R_2 &= \cdot 406 \text{ ,,} & R_2^2 &= \cdot 1648 \\ R_3 &= \cdot 922 \text{ ,,} & R_3^2 &= \cdot 8500 \\ R_4 &= \cdot 969 \text{ ,,} & R_4^2 &= \cdot 9389 \\ R_2^2 - R_1^2 &= \cdot 0859 \text{ ,,} & R_4^2 - R_3^2 &= \cdot 0889 \end{aligned}$$

Area of cross section of inner member =

$$\pi (R_2^2 - R_1^2) = \cdot 2689 \text{ sq. in.} = \frac{1}{4} \text{ sq. in., nearly.}$$

Area of cross section of outer member =

$$\pi (R_4^2 - R_3^2) = \cdot 2793 \text{ sq. in.} = \frac{1}{4} \text{ sq. in., nearly.}$$

Also, we have—

$$\begin{aligned} \frac{R_3}{R_2} &= 2\cdot 270, & \frac{R_2}{R_1} &= 1\cdot 444, & \frac{R_4}{R_3} &= 1\cdot 051, \\ \frac{R_1^2}{R_2^2 - R_1^2} &= \cdot 9185, & \frac{R_4^2}{R_4^2 - R_3^2} &= 10\cdot 561, \\ \frac{2 R_1^4}{(R_2^2 - R_1^2)^2} &= 1\cdot 6872, & \frac{2 R_4^4}{(R_4^2 - R_3^2)^2} &= 223\cdot 068, \\ \log_e \frac{R_3}{R_2} &= \log_{10} 2\cdot 27 \times 2\cdot 3026 = 2\cdot 3026 \times \cdot 35603. \end{aligned}$$

These quantities are required in the calculation of the inductance of the cable.

If the specific inductive capacity of the dielectric were a definitely known quantity, it would be, of course, easy to calculate from the above figures the capacity of one mile run of the cable by the formula,

$$\text{Capacity in electrostatic units} = \frac{K l}{2 \log_e \frac{R_4}{R_2}},$$

K being the specific inductive capacity, and  $l$  the length; and to reduce this to farads by multiplication by  $9 \times 10^{11}$ .

In default of information to enable this to be done, the electrostatic capacity of one length was measured directly by comparison with a standard half a microfarad.

On December 5th, 1890, Mr. Ferranti placed at my disposal two lengths of the trunk mains running from Trafalgar Square to Blackfriars, for the purpose of measuring the capacity. The length of cable used was 2,399 yards, or 1·363 statute miles. The capacity of one of these lengths (No. 3 cable) was found in the usual way (taking the capacity between the inner and outer members) to be ·5 microfarad for the whole length, or at the rate of ·367 microfarad per statute mile. It follows from this that the specific inductive capacity of the material used is about 3·4 (air = 1). Using the other cable (No. 4), the copper resistance was found to be ·324 legal ohms, or ·328 B.A. units per statute mile, reckoning the resistance per mile run of the cable, and including, therefore, both the inner and outer conductor. The insulation resistance between the inner and outer member of the first cable was found to be at the rate of 720 megohms per mile. The instruments used in taking these measurements were a half-microfarad condenser by Dr. Muirhead, and a legal ohm Wheatstone bridge and megohm standard, kindly lent by Messrs. Elliott Bros. for the purpose. The temperature of the ground on that day was near the freezing point, and Nature saved us the trouble of reducing the values to zero centigrade.

The remaining element required is the inductance of the cable per mile. Fortunately, the form of cable which is the best for practical reasons, is almost the only case in which the inductance can with no great difficulty be calculated with any required degree of accuracy from the dimensions of the cable. Lord Rayleigh has solved for us the problem of the inductance of a pair of concentric conductors placed co-axially. His formula, as given in his paper in the *Philosophical Magazine* of May, 1886, page 385, is as follows, when translated into my nomenclature, and the values of the magnetic permeabilities of the materials all taken as equal to unity :—

$$L = \text{inductance per unit of length} = 2 \log \frac{R_3}{R_2} + \frac{2}{R_2^2 - R_1^2} \left( \frac{R_2^2 - 3R_1^2}{4} + \frac{R_1^4}{R_2^2 - R_1^2} \log \frac{R_2}{R_1} \right) + \frac{2}{R_4^2 - R_3^2} \left( \frac{R_3^2 - 3R_4^2}{4} + \frac{R_4^4}{R_4^2 - R_3^2} \log \frac{R_4}{R_3} \right).$$

The logarithms are all Naperian, and the values of  $R_1$ ,  $R_2$ ,  $R_3$ , and  $R_4$  may be taken in any units, as only their ratios are required.

The values of the chief functions of the radii of the tubes have been given above, and, making the substitutions, we have for the inductance of one centimetre run of the cable the following value:—

$$L = 2 \log 2.27 + 1.6872 \log 1.444 + 223.068 \log 1.051 - (9185 + 10.561).$$

Remembering that the logarithms here are Naperian, and that the logarithms to base 10 must be multiplied by 2.3026 to reduce them to the Naperian base, we have as a final result of the calculation,

$$L = 1.7755 \text{ centimetres.}$$

Hence, since one statute mile = 160,931.5 centimetres, the inductance per statute mile of the Ferranti cable is 285,734 centimetres = .0002857 of a henry, or 285.7 microhenries.

This result will be accurate to just the extent to which the diametrical measurements of the conductors are accurate.

Collecting our results, we have for one statute mile run of the Ferranti trunk main the following values, as deduced from measurements on the particular lengths employed:—

$$R = \left\{ \begin{array}{l} \text{Copper resistance at} \\ 0^\circ \text{ cent. of lead and} \\ \text{return} \quad \dots \quad \dots \end{array} \right\} = \left\{ \begin{array}{l} .324 \text{ legal ohm per} \\ \text{mile.} \end{array} \right.$$

$$K = \left\{ \begin{array}{l} \text{Insulation resistance} \\ \text{of length measured} \\ \text{at rate of} \dots \dots \end{array} \right\} = \left\{ \begin{array}{l} 720 \text{ megohms per} \\ \text{mile between} \\ \text{inner and outer} \\ \text{conductor.} \end{array} \right.$$

$$C = \left\{ \begin{array}{l} \text{Electrostatic capacity} \\ \text{between inner and} \\ \text{outer conductor} \dots \end{array} \right\} = \left\{ \begin{array}{l} .367 \text{ microfarad per} \\ \text{mile.} \end{array} \right.$$

$$L = \text{Inductance} \dots \dots = \left\{ \begin{array}{l} 285.7 \text{ microhenries} \\ \text{per mile.} \end{array} \right.$$

The formula given above for the inductance per unit of length can be applied to stranded concentric cables with a fair degree of approach to accuracy.

Suppose that we are sending periodic currents having a frequency of 67 through the Ferranti cables, then, if  $n =$  frequency  $= 67$ ,  $2 \pi n = 421 = p$ , and  $4 \pi^2 n^2 = p^2 = 177,241$ .

Hence, since  $R = .324$  ohm, and  $L = .000286$  henry, we have  $\sqrt{R^2 + p^2 L^2} = .3455$  ohm for the impedance per mile, and we see that at this frequency the impedance exceeds the resistance by about 7 per cent. of the latter.

From the data thus given other functions of the capacity,  $C$ , inductance,  $L$ , resistance,  $R$ , and impedance,  $I$ , per mile can be built up, which will be required presently. These are—

$$P^2 = \frac{C p}{2} \left\{ \sqrt{R^2 + p^2 L^2} - p L \right\} = .00001738;$$

$$Q^2 = \frac{C p}{2} \left\{ \sqrt{R^2 + p^2 L^2} + p L \right\} = .00003597.$$

Hence, also,

$$P = .004168, Q = .005997;$$

$$P^2 + Q^2 = .00005335, \frac{P^2 + Q^2}{I^2} = \frac{447}{10^4};$$

$$(2 P R + 2 Q L p) = .004147;$$

$$(2 P L p^2 - 2 Q p R) = -1.216.$$

All the quantities refer to one mile run of the cable.

15. In the course of various experiments during the laying of the cable, it became evident that a conductor having so considerable an electrostatic capacity behaved in a curious manner under some circumstances when long lengths were connected up to a transformer. This behaviour has been much misunderstood, and much misdescribed. Remarkable phrases have been coined with reference to it, such as "the piling up of the volts," or "packing up of the volts," which are not very lucid descriptions of what really does occur. Ingenious theories have been constructed also to account for phenomena which do not exist. Briefly stated, it appears to be as follows:—When an insulated and sufficient length of the cable has its inner and outer members connected to the terminals either of a

transformer or of an alternator having an armature of sufficient inductance, it is observed that under these conditions the potential difference between the inner and outer members, measured anywhere along the length of the cable, is generally greater than would be the potential difference of the terminals of that alternator or transformer if the cable were very short, or not present at all. This seems to hold good also when the far ends of the inner and outer members are connected together through a not very small resistance, such as a row of incandescent lamps in series, or a high-resistance voltmeter, or transformer lightly loaded. The cable acts in all apparently as one single condenser would do, and in no case, as far as is known, is there evidence of a continuous rise of pressure along the trunk mains themselves. The pressure at the London end is not found to be greater than at the Deptford end. The condenser effect is increased apparently by increase of length of the main, at least within observed limits, and in all cases the main acts as if it were a condenser bridged across between two parallel mains without capacity. When a current flows through the cable there is an electric flux across the dielectric between the inner to outer members which constitutes the *condenser current* of the mains, and the outflowing current is in general less in magnitude than the inflowing current, although not always necessarily so. The current flowing out at the London end may be called the *work current*. There is a difference in phase between the work current and the condenser current. The ingoing current is the resultant of the work and condenser current.\*

16. In order to examine the phenomena of these trunk mains more closely, some experiments were made on one long length of nearly 12 miles of the cable by joining the conductors of two

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\* Previous discussions on the subject of this effect, rather misnamed the "Ferranti effect," occurred at the Physical Society during the discussion on a paper by Mr. Swinburne on "Alternate-Current Condensers," and was continued by Mr. Swinburne, Mr. Glazebrook, Professor Ayrton, and Mr. Kapp, in various articles and papers, for which the reader is referred to the *Electrician*, vol. xxvi. Dec. 19, 1890, p. 214; Dec. 26, p. 232; Jan. 2, 1891, p. 260. Mr. Gisbert Kapp wrote an article on "Capacity and Self-Induction in Alternate-Current Working," published in the *Electrician* for Dec. 19, 1890, p. 197, and Dec. 26, p. 229, in which condenser effects are discussed with his usual clearness and care.

trunk mains in London, and having both ends of this gigantic loop of concentric cable at our disposal in the Deptford station. These experiments were arranged by Mr. Ferranti to give Mr. Preece, Major Cardew, and me the opportunity of obtaining measurements of these condenser effects in the Ferranti trunk mains. The experiments were made on Friday, January 9th, 1891, at Deptford, and Mr. Ferranti, Mr. Sparks, Mr. D'Alton, Mr. McLean, and the assistant engineers took part in carrying out these measurements. The arrangements were made as follows (see Fig. 10):—One of the 1,200-H.P. dynamos (No. 2) was set

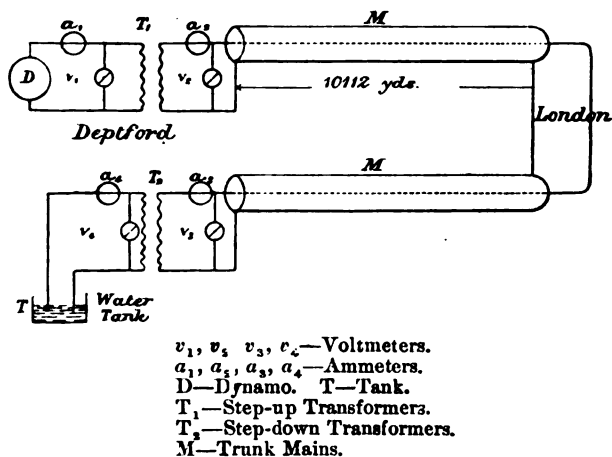


FIG. 10.

apart for the purpose, and excited so as to supply current at about 2,500 volts or so. The current from this dynamo was led through the primaries of two 150-H.P. transformers joined in parallel, and which raised the pressure from 2,500 to 10,000 volts. At this pressure a current from the secondaries was sent into the concentric cable at Deptford, thence to the sub-station in Trafalgar Square in London by one main, and back to Deptford on the other trunk main. This total distance is 20,224 yards, or 11.49 statute miles. Arriving back at Deptford, the current was again reduced from 10,000 to 2,500 volts, and the energy taken up in water resistance in a water tank at the latter pressure. The pressure at different parts of the system was measured by Cardew voltmeters,

the high pressures being reduced by 100 : 1 pilot transformers. The currents were measured by Evershed ammeters. The current and pressure were measured at four places—viz., at the terminals of the dynamo, at the beginning, or entrance, of the trunk main, at the end of the trunk main, and, after reduction, at the tank. Observers stationed at these different instruments took simultaneous observations at a signal given by the sound of a whistle, and in this way the simultaneous values of the pressure and current were obtained at these four places. The transformers used for raising and lowering the pressure were the 150-H.P. size, having on the primary side 1,120 turns of wire, of resistance 3.5 ohms, and on the secondary side 280 turns of wire, having a resistance of .2442 ohms. Hence these transformers were 4 : 1 transformers. There were, in addition, a pair of pilot transformers, which had a change ratio of 100 : 1, and which reduced from 10,000 to 100 volts for voltmeter purposes. The observations consisted in exciting the dynamo to various voltages, and observing the current and pressure at the four places named, whilst different loads were placed on the system by taking up more or less current in the tank.

17. From a large number of observations the following are selected as showing the general results obtained, and are set forth in Table I. The first column gives a number for each experiment. The second and third give the pressure and current at the dynamo terminals. The fourth column is a series of numbers which are four times those in the second column, and are the pressures to which the 1 : 4 step-up transformers at the home end would raise the dynamo pressure if the cable were absent. The fifth and sixth columns give the pressure and current at the home, or dynamo, end of the trunk main, and it will be seen that in every case this observed pressure is greater than four times the pressure at the dynamo terminals. The seventh and eighth columns give the pressure and current at the far end of the trunk main, and it will be seen that in every case the current flowing out of the main is less than that going into it. Columns ten and eleven give the pressure and current after reduction by the step-down transformers, at which pressure the energy was taken up on

## EXPERIMENTS ON THE PRESSURE DISTRIBUTION IN THE FERRANTI TRUNK MAINS.

I No. of Experiment.	II At Dynamo Terminals.		V. Dynamo Pressure multiplied by 4.	V. At Home End of Trunk Main.		VI. At Far End of Trunk Main.		IX. Pressure at Tank multiplied by 4.	X. At the Tank.		XI. Current.
	Pressure.	Current.		Pressure.	Current.	Pressure.	Current.		Pressure.	Current.	
1	2,177	121.4	8,708	9,060	29.2	8,950	26.5	8,640	2,160	118.2	
2	2,386	119.4	9,544	10,100	31.5	9,770	28.0	9,504	2,376	117.1	
3	2,458	119.4	9,832	10,300	32.0	10,120	28.0	9,840	2,460	116.0	
4	2,177	119.3	8,708	9,110	29.2	8,930	26.5	8,640	2,160	116.0	
5	2,311	113.2	9,244	9,700	28.7	9,570	24.8	9,408	2,352	105.5	
6	2,251	110.2	9,004	9,600	27.6	9,280	24.3	9,024	2,256	105.5	
7	2,568	106.1	10,272	10,490	27.0	10,070	23.3	9,792	2,448	101.6	
8	1,994	108.1	7,976	8,420	26.0	8,200	23.3	7,872	1,968	100.7	
9	2,448	100.0	9,792	10,170	24.0	10,690	19.0	10,128	2,532	56.6	
10	2,496	92.8	9,984	10,560	22.0	10,590	17.0	10,512	2,628	56.0	
11	2,400	92.8	9,600	10,360	21.5	10,490	16.3	10,224	2,556	55.5	
12	2,311	64.1	9,244	9,700	17.0	9,770	Not taken	9,600	2,400	42.2	
13	2,280	41.7	9,120	9,870	11.0	9,900	0	9,756	2,434	0	
14	2,328	41.7	9,312	9,970	11.0	10,000	0	9,880	2,470	0	
15	1,620	42.7	6,480	7,240	10.0	7,350	0	7,248	1,812	0	
16	1,104	24.4	4,416	5,200	5.0	5,200	0	4,964	1,241	0	
17	1,212	24.4	4,848	5,500	9.9	5,500	0	5,456	1,389	0	
18	1,308	24.4	5,232	5,660	9.0	5,900	0	5,740	1,435	0	
19	1,800	37.6	7,200	7,920	10.0	7,920	0	7,784	1,946	0	
20	2,136	48.8	8,544	9,110	12.0	9,410	0	9,244	2,311	0	

water resistance in a tank. Column nine gives the pressure on the primary, or cable, side of the 4 : 1 step-down transformers, which would be necessary to give the pressure observed at the tank if the cable were not present.

18. We have then to see how these observed values agree with theory. It must be, however, stated that these numbers and values so observed are not minutely accurate. Owing to the fact that the speed of the dynamo varied slightly, and to the fact that the Cardew voltmeters used were set vertical, and not horizontal, some degree of inaccuracy exists in the readings of current and pressure, but the figures are perhaps sufficient to test the general agreement of fact with theory.

We note, first, that in the experiments at full load, when 118 amperes was being taken up in the resistance, the load on the transformers was about 340 H.P., and hence the two 150 H.P. transformers at each end joined in parallel were slightly overloaded. Under these circumstances, the inductance of the transformers at the step-up end is reduced practically to a very small value, and we may consider that the actual current of 30 amperes or so flowing in the cable can be considered to be the resultant of two superimposed currents differing about 90 degrees in phase from one another. There is, first, the *work* current of 26.5 amperes in Experiment I. which is flowing out of the cable, and which is practically in synchronism with the impressed electro-motive force in the secondary circuit of the step-up transformers. Then there is the *condenser* current of the cable considered as one single condenser, which will be 90 degrees in advance in phase of the impressed electro-motive force creating this charge. Hence the values of the ingoing currents given in column VI. are the resultants of the outgoing currents given in column VIII., and the condenser current of the cable, which is in quadrature with the last. Hence the magnitude of the condenser current must be the square root of the difference of the squares of the currents given in columns VI. and VIII., which may be called respectively the *resultant current* and the *work current*.

Taking the first six experiments in Table I., we calculate

from them the square root of the difference of the squares of the currents in columns VI. and VIII. of Table I., and write them as in Table II. below.

Table II.

Resultant Current going into Cable = $a$ .	Work Current coming out of Cable = $b$ .	Condenser Current of Cable = $\sqrt{a^2 - b^2}$ .
Amperes.	Amperes.	Amperes.
29.2	26.5	12.2
31.5	28.0	14.4
32.0	28.0	15.5
29.2	26.5	12.2
28.7	24.8	14.4
27.6	24.3	13.1

Unfortunately, the speeds of the dynamo were not taken at each observation; but the normal frequency of the current from these dynamos is 67, and in these experiments the dynamo speeds were marked as rather below the normal. If  $n = 67$ ,  $2\pi n = p = 421$ . It is probable that the value of  $p$  was not greater than 400 in these experiments. We have seen (equation 2, *ante*) that if  $v$  is the instantaneous potential difference of the condenser terminals, and  $i$  the current flowing into it at any instant,  $i = C \frac{dv}{dt}$ . Hence, on the assumption of a simple periodic variation of the current, we may write  $p$  for  $\frac{d}{dt}$ , and put as the value of the square root of the mean square of the condenser current,  $I$ , the expression,

$$I = p C V,$$

where  $C$  is the capacity, and  $V$  is the potential difference of the terminals of the condenser as read by an electrostatic or Cardew voltmeter. It then follows that the capacity of the cable is determinable from the values of the condenser current,  $I$ , the

pressure,  $V$ , as read at the step-up end, and from the known value of  $p$ .

We may examine how far the capacity value so deduced agrees with measurement.

We tabulate in columns the values of the condenser currents,  $I$ , found from Table II., and set against these the mean potential difference of the inner and outer members of the cable as the mean potential difference of the surfaces of the condenser. Calling these last values  $V$ , we then calculate the capacity,  $C$ , in microfarads from the formula,

$$C = \frac{I}{p V} \times 10^6,$$

on the assumption that  $p = 400$ . These results are set down in Table III. The values of the potential,  $V$ , are the means of the pressures given in columns v. and vii. of Table I. as the pressures at the step-up and step-down end of the trunk main.

*Table III.*

Condenser Currents, $I$ , in Amperes, taken from Table II.	Mean Potential Difference of Condenser Surfaces, $V$ , of Cable, in Volts.	Calculated Capacity for the whole Length of Cable, in Microfarads.
12.2	9,005	3.4
14.4	9,935	3.6
15.5	10,210	3.8
12.2	9,020	3.4
14.4	9,635	3.7
13.1	9,440	3.5

The mean electrostatic capacity of the 11.5 statute miles of the cable in use is therefore about 3.6 microfarads, as deduced from these experiments. On the day following these experiments the electrostatic capacity was measured at Deptford by Mr. H. R. Kempe, using a microfarad standard and a battery of about 18 Daniell cells, and taking galvanometer throw observa-

tions in the usual way. Mr. Kempe found the capacity under these circumstances of the respective mains to be about 1.98 and 2.02 microfarads, or in all about 4 microfarads. Accordingly, we see that the capacity, as deduced from the mean calculated condenser currents and the observed pressures at the ends of the mains, comes out a little low compared with the measured steady value; but the speeds and pressures were hardly observed with sufficient accuracy to make a closer agreement very likely, and the condenser current values are probably affected by some errors. It may be observed that the effect of a small but definite inductance in the fine-wire circuit of the transformers at the step-up end existing unannulled at full load would be to lower the condenser currents, and hence to lower the calculated capacity. I think it is pretty clear that what may be called the normal condenser current of the 11.5 statute miles of the trunk mains is about 14 or 16 amperes under a working pressure of 10,000 volts at the step-up end of the main, and at a frequency of about 67 for the circuit. This is what is generally observed.

On one occasion, when the writer was engaged with Mr. Ferranti in testing the trunk mains, we had a similar arrangement of the system to that above described—viz., a length of 11.5 statute miles of trunk main looped in London through two step-down and step-up transformers in parallel, and a similar set of transformers at Deptford.

Two 100-candle-power lamps were put on the far end of the system at Deptford, just to give indication of a current flowing through the system, otherwise there was no load. When the dynamos were excited so as to give 2,500 volts on the primary side of the step-up transformers, and this raised to 10,000 volts in the mains, we found a current of about 64 amperes going out of the dynamo at 2,500 volts. This would correspond to 16 amperes or so in the trunk mains. The actual working current at this time was merely the magnetising current of the step-down transformers and the normal current required for the lamps. Hence this experiment agreed in showing that the normal condenser current of the trunk mains at 10,000 volts pressure and a frequency of 67 is about 1.4 amperes per mile run of the cable.

19. Let us next examine how far the observations taken with no load on the secondaries of the step-down transformers agrees with the foregoing. In this case we restrict our view to the last eight observations recorded in Table I. The numbers in column iv. give us at once the condenser current of the cable, because there is no sensible current flowing out of the cable except the negligible magnetising current of the transformers.

*Table IV.*

Condenser Current, I, in Amperes.	Mean Potential Difference of Condenser Surfaces of the Cable, in Volts.	Calculated Capacity for the Whole Length, in Microfarads.
11	9,885	2·8
11	9,985	2·8
10	7,295	3·4
5	5,200	2·4
9	5,500	4·1
9	5,900	3·8
10	7,920	3·1
12	9,395	3·2

The variation in the values of the capacity shows that the measurements of the condenser currents are probably affected by considerable errors; and this is likely, seeing that the ammeters on the end of the trunk main would not read with sufficient accuracy to detect a difference of one or two amperes at these low readings. Hence all we can say is, that the mean capacity of the cable as deduced from the observations at no load—viz., 3·3 microfarads—is not extravagantly different from that obtained by observations at full load.

If we take one of the observations at half load—say No. 10 in Table I.—we arrive at the same result. For the ingoing and outgoing currents in the trunk main are then 22 and

17 amperes, and the difference of their squares is 14, which is probably nearly the condenser current in this case; and the mean potential difference of the surfaces of the cable is 10,560 volts; and hence, as before, the capacity comes out 3.3 microfarads for the whole length. Hence all the observations agree in giving a capacity of from 3.3 to 3.6 microfarads for the total cable in use. It may well be questioned whether for this paraffined-paper dielectric the capacity as determined by a prolonged charge and discharge gives a value which is correctly expressive of the true working capacity of the cable under rapid alternations of charge. It must be remembered that paraffined paper presents usually a considerable absorption effect, and the value of 4 microfarads for the 11.5 miles, obtained by the usual ballistic galvanometer method, may be, and perhaps is, a value which is higher than that which correctly expresses the true working capacity of the cable under rapidly reversed electric stresses. It is important to have this point investigated for the dielectrics in use in the manufacture of types of concentric cables now employed.

20. Passing to the examination of other points indicated by the observations in Table I., we see that the mean potential difference of the two conductors of the cable is always greater than four times the dynamo terminal pressure as given in column iv. That is to say, these figures show that the pressure in the trunk main at the dynamo end is always greater than would be the pressure at the terminals of the step-up transformers if the trunk main were disconnected. This may be said to be the fundamental phenomenon which has been much dis-described and misunderstood. The values of the pressures as read at the dynamo terminals and at the two ends of the trunk mains show that whenever the system is loaded, and there is a fairly heavy load, there is a fall of pressure down the cable, as there should be; although the actual figures seem to show that this fall of pressure is not quite equal in value to the product of the values of the true resistance of the cable and the current flowing through it. Those cases in which the figures in Table I. seem to show a rise of pressure along the main cannot be accepted as representing the

actual facts. When the cable is relieved of all working current, and only traversed by a condenser current, the figures show that the cable is either all at one pressure all along, or else that there may be a slight rise from the step-up end to the step-down end of the cable. I think, however, that there is no good evidence of this rise of pressure from end to end of the cable when not loaded with external load. The chief fact is that, whereas the step-up transformers would, if existing alone, raise the dynamo pressure fourfold, we find that when the cable is heavily loaded the mean potential difference of the conductors exceeds four times the dynamo terminal pressure by something like 5 per cent.; and, when the external load is removed, that excess pressure more nearly amounts to about 10 or 15 per cent.

The general fact is quite in accordance with the foregoing theory. When the step-up transformers are heavily loaded, the inductance of their secondary circuits is practically annulled or very much reduced; when they are lightly loaded, it is at once increased. We have seen, in discussing the clock-diagram of electro-motive forces as applied to this condenser problem, that the increase in pressure which takes place is always an evidence of the presence of inductance in the charging circuit, and that this rise of pressure will augment with increase of inductance in the line or in the step-up transformer. We are unable to test how far the general rise of pressure is in accordance with the foregoing theory, because we have no means of measuring the true mean inductance of the secondary, or high-pressure, side of the step-up transformers. The presence of capacity in the line always keeps down the inductance of the transformer, because it compels a rather large current to flow into it from the dynamo, even when the work current flowing out of the cable is zero, or nothing more than the magnetising current of the step-down transformers.

21. In the foregoing paragraphs no attempt has been made to enter into a discussion of the physical *modus operandi* of these condenser effects. We can easily construct, as has been frequently done, mechanical analogies, in which the elastic yielding of a spring and the inertia of a heavy body take the

place of the capacity and inductance in the electric system. It is possible, however, to overrate the value of this mechanical parallel. It is probable that the actual physical operations taking place to produce these electrical effects are not adequately represented by a mechanical simile in which the inertia-like aspect of self-induction is exclusively regarded, although, of course, such mental representations have their use. Too much hypothesis is often as bad as too little in seeking to connect together observed facts and fundamental principles. We may regard a condenser, when subjected to periodic currents, as a non-dissipative impedance, if the term may be allowed, having also a negative self-induction. A condenser allows a flux of electricity to take place backwards and forwards through its dielectric, and, as far as alternating currents are concerned, it may be said to have a conductivity for electricity.

A condenser of capacity  $C$ , when traversed by periodic condenser currents of frequency  $n$ , acts in any circuit as equivalent to an impedance of magnitude  $\frac{1}{Cp}$ . It also acts as if it had a negative inductance equal to  $\frac{1}{Cp^2}$ ,  $p$ , as usual, standing for  $2\pi n$ . By a negative inductance  $\frac{1}{Cp^2}$  must be understood a power of nullifying positive inductance in a conductor in series with the condenser numerically to the amount of  $\frac{1}{Cp^2}$  henries. Suppose, then, that a condenser of capacity  $C$  is placed in series with an inductive resistance of inductance  $L$  and resistance  $R$ , and a periodic P.D. equal to  $V_0$  permitted to act on the two in series. The resultant inductance of the whole circuit is  $L - \frac{1}{Cp^2}$  and the resultant impedance is

$$\sqrt{R^2 + \left(L - \frac{1}{Cp^2}\right)^2 p^2}.$$

Hence the current through the condenser is equal to

$$\frac{V_0}{\sqrt{R^2 + \left(L - \frac{1}{Cp^2}\right)^2 p^2}}.$$

If the potential difference of the two sides of the condenser is called  $V_1$ , and the equivalent impedance of the condenser is  $\frac{1}{Cp}$ , we may consider that the value of  $V_1$  is obtained by taking the product of this so-called impedance of the condenser,  $\frac{1}{Cp}$ , and the value of the current flowing through it. Hence we have,

$$V_1 = \sqrt{R^2 + \left(L - \frac{1}{Cp^2}\right)^2 p^2} \frac{1}{Cp};$$

but this is the same expression as that deduced before, for the ratio of  $V_1$  and  $V_0$ . This is not to be considered as a demonstration of the equation, but only as showing how it may be regarded as being built up on the assumption that a condenser behaves towards alternating currents as an impedance causing fall of potential in a current passing through it, and capable of nullifying inductance in a circuit connected in series with it; and that such a view leads to an equation from which it is immediately seen that under some conditions  $V_1$  must be greater than  $V_0$ .\*

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\* Mr. O. Heaviside points out (in a private letter) that, if a condenser has a true dielectric conductivity  $K$ , and a capacity, or permittance, as he call it,  $C$ , then for simple periodic currents of frequency  $n = p/2\pi$  the condenser acts as if it were a coil of resistance  $R'$  and inductance  $L'$ , where

$$R' = \frac{K}{K^2 + C^2 p^2}, \text{ and } L' = \frac{-C}{K^2 + C^2 p^2}.$$

The reciprocal of the impedance of the condenser, which he calls the "admittance," is  $(R'^2 + p^2 L'^2)^{-\frac{1}{2}} = (K^2 + C^2 p^2)^{\frac{1}{2}}$ . Hence, for coils and condensers we have the following qualities, which are analogous:—

<i>Coils have</i>	<i>Condensers have</i>
<i>Resistance</i> $R$ .	<i>Conductance</i> $K$ .
<i>Inductance</i> $L$ .	<i>Permittance</i> $C$ .
<i>Impedance</i> $(R^2 + p^2 L^2)^{\frac{1}{2}}$ .	<i>Admittance</i> $(K^2 + p^2 C^2)^{\frac{1}{2}}$ .

For condensers having no sensible true conductance (perfect condensers)  $K = 0$ , and the inductance becomes  $-\frac{1}{Cp^2}$ , and the impedance  $\frac{1}{Cp}$ , as stated in the text. These statements are obviously consequences deducible from the pair of fundamental differential equations for "inductive" and "permittive" circuits, viz.:—

$$L \frac{di}{dt} + Ri = v,$$

and

$$C \frac{dv}{dt} + Kv = i;$$

whence everything else is deduced. For further conceptions on resistance and

22. Reference has been made recently in a paper read by Major Cardew to the difference of electric condition as regards potential above the earth which will exist between two insulated conductors one of which entirely encloses the other, and which have different capacities, when they are connected to the terminals of an insulated transformer or alternator. In the case of a concentric cable the two copper members of which are insulated from each other, and the outer one insulated from a metallic sheath connected to earth, we have two insulated conductors, the interior one having capacity with reference to the outer, but not with reference to the earth, and the outer one having capacity both with reference to the inner and also with respect to the earth. This last capacity is generally, in cables as actually made, very much greater than the former.

In the case of the Ferranti trunk mains, the inner-to-outer capacity is, as we have seen, about one-third of a microfarad per mile run of the cable. Calculating from the measured dimensions, and from the specific inductive capacity of the insulating material as deduced from the inner-to-outer capacity, the magnitude of the outer-to-earth capacity I find to be about ten times greater, or more than three microfarads per mile. It follows immediately, from elementary principles, that the two insulated members of such a concentric cable will, when connected respectively to the terminals of an insulated transformer or dynamo, always be brought into such a condition that the outer member is at zero-potential as regards the earth, and that the full difference of potential which the transformer or dynamo is capable of making will subsist between the outer and inner member of the cable. This is true only when the

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conductance operators, the reader is referred to a paper by Mr. Heaviside in the *Philosophical Magazine* for December, 1887. The term "impedance" has now become absorbed into practical science. If at any future time alternating currents of very high frequency become employed in electro-technical work, the capacity effects of circuits will make themselves more evident to the practical man, and then the quantity above called the "admittance" will be forced into notice, and become a candidate for notice and for name. When working at very slow alternations, the *resistance* of the circuit is its most important quality. At higher alternations the *inductance* forces itself into view, and at higher still the *capacity* can by no means be ignored.

outer member entirely encloses the inner member; but it is also true that when the two conductors have different capacities, by reason of position or form, there will be an electrical inequality between them, in that the difference of potential between each conductor and the earth will not be the same. The conductor which has the larger capacity will have the lesser potential with respect to the earth, provided always that both conductors attached to the poles of the generator or transformer are insulated throughout their length.

23. The condenser effect we have been considering, and in particular the alteration of the change ratio of a transformer which takes place when its secondary circuit is connected to a conductor system having a certain capacity in relation to the inductance of that secondary circuit, is found to operate in many curious ways in the practical use of such combinations. For instance, if a pair of alternators separately excited are working in parallel on an omnibus main leading to a conductor system of no sensible capacity, then we know very well that if the dynamos are synchronised, and contributing equally to the outgoing current, the method adopted to take one dynamo out of circuit would be to lower its exciting current, and when its outgoing current fell to zero to sever its connection with the omnibus main. If, however, the alternators are working through step-up transformers, and into a conductor system consisting of concentric cable, it is found by experience that the exciting current of the dynamo to be taken out must be raised, and not lowered, in order to bring it into a condition in which it can be cut off from the omnibus main without dangerous sparking at the switch; and, conversely, in putting it into parallel the incoming alternator has to be excited to a lower electro-motive force than is necessary to maintain the standard P.D. at the terminals of the step-up transformer before putting it into connection with the omnibus main. This is obviously due to the fact that the change ratio of the step-up transformer is altered by the very act of putting it into connection with the conductor system, and hence the excitation of the alternator at the moment of making or breaking connection

with the main must be such as will bring the P.D. of the free secondary terminals of the step-up transformer to a value equal to the P.D. of the two points on the conductor having capacity to which they are about to be connected, or from which they have just been severed; and when the connection of the alternator is made, the excitation can be altered to divide the load on the machines properly. This, of course, would not be the case if high-tension alternators of very small inductance were set to work in parallel on such a concentric conductor system. In this case the condenser effect is practically nothing, and the behaviour of the machines on going into or out of parallel would present nothing unusual.

24. In the cases briefly discussed above, the cables, whether concentric or simple, have been found to act as single condensers substituted for them would act; and in the experiments described no progressive rise of pressure from one end to the other of a cable possessing capacity was found to exist.

It is, however, quite possible for self-induction, resistance, and capacity to be so combined together in one conductor that the action of an impressed periodic electro-motive force at one end will create a progressive rise of mean pressure along the cable when the far end is insulated. The complete theory of such effect is rather too long to discuss here. The propagation of a simple periodic current and the action of a simple periodic electro-motive force upon a concentric cable has been very fully discussed by Mr. Oliver Heaviside in an able series of papers on "The Self-Induction of Wires," published in the *Philosophical Magazine* for 1886 and 1887. A very brief summary of the results is all that can be given here; but those desirous of further information may be particularly referred to *Philosophical Magazine*, vol. xxiii., January, 1887, page 18, in which paper Mr. Heaviside deals with the problem which is particularly the practical one, viz., a concentric cable having at one end a transmitting apparatus, such as a telephone, alternator, or a step-up transformer, and at the other a receiving instrument, such as another telephone or a step-down transformer.

We have, then, a sending instrument having resistance and

inductance ; a cable having resistance, capacity, and inductance ; and a receiver having also resistance and inductance. Given the electro-motive force in the transmitter, the problem is to find the current in the receiver.

Consider, first, a very long concentric cable having inductance  $L$ , resistance  $R$ , and capacity  $C$ , all per unit of length. Fix attention upon a small length of the cable,  $\delta x$ , and suppose that the current flowing into this element at any instant is  $i$ , and the current flowing out of it is  $i - \delta i$  when some electro-motive force acts at one end of the cable. Also, let  $v$  be the potential of the inner conductor at the one end of the element of length,  $\delta x$ , and  $v - \delta v$  be the potential at the other end : then the current,  $i$ , at any instant,  $t$ , is determined from the fundamental equation,

$$L \delta x \frac{d i}{d t} + R \delta x i = \delta v \quad \dots \quad \dots \quad (33)$$

and the current and potential are connected by another relation,

$$\frac{d i}{d x} = C \frac{d v}{d t} \quad \dots \quad \dots \quad \dots \quad (34)$$

because the current flowing out of the element of length is less than the current flowing into the element by the condenser current taken up in the element.

Differentiating (34), and substituting in (33), we arrive easily at the equation,

$$L C \frac{d^2 v}{d t^2} + R C \frac{d v}{d t} = \frac{d^2 v}{d x^2} \quad \dots \quad \dots \quad (35)$$

This differential equation gives us the means of determining the potential,  $v$ , at any time and place in the cable.

Let  $v_0$  be the electro-motive force at the origin or the end of the cable at which the dynamo or electromotor is applied, and let  $V_0$  be its maximum value—such, that,

$$v_0 = V_0 \sin p t :$$

then, if the cable is infinite in length, the solution of the equation (35) can be obtained, and the potential,  $v$ , at any distance,  $x$ , from the origin, and any time, is easily shown to be given by

$$v = V_0 e^{-r x} \sin (p t - Q x),$$

where the quantities  $P$  and  $Q$  are—

$$P = \pm \left\{ \frac{p C}{2} \left( \sqrt{R^2 + p^2 L^2} - L p \right) \right\}^{\frac{1}{2}},$$

$$\text{and } Q = \pm \left\{ \frac{p C}{2} \left( \sqrt{R^2 + p^2 L^2} + L p \right) \right\}^{\frac{1}{2}}.$$

For the Ferranti trunk mains the values of these quantities have been calculated in Section 14.

If the cable is not infinite in length, but has a finite length,  $l$ , then two cases may be considered—(i.) when the cable is short-circuited at the far end, and (ii.) when its two members are insulated or open-circuited at the far end.

Taking the first case of a concentric cable of length  $l$ , short-circuited at the far end, and attached to an alternator of negligible inductance and resistance at the other, Mr. Heaviside has shown how the potential at any distance,  $x$ , from the machine can be obtained, and also the current flowing through an ammeter placed at any point in the cable. If  $V_0$  is the maximum value of the periodic electro-motive force applied at the dynamo end, and  $v$  is the potential at any time,  $t$ , and any distance,  $x$ , from the dynamo end, then he gives the value of  $v$  as

$$\begin{aligned} v &= V_0 e^{-Px} \sin (pt - Qx) \\ &+ V_0 \frac{e^{Px} \sin (pt + \theta_2 + Qx)}{e^{Pl} (e^{2Pl} + e^{-2Pl}) - 2 \cos 2Ql} \\ &- V_0 \frac{e^{-Px} \sin (pt + \theta_2 - Qx)}{e^{Pl} (e^{2Pl} + e^{-2Pl}) - 2 \cos 2Ql} \end{aligned}$$

(see *Phil. Mag.*, vol. xxii., September, 1886, p. 284).

It can be shown from the above that the ratio of the maximum value,  $V$ , of the potential at any distance,  $x$ , from the origin, to the potential at the origin,  $V_0$ , is given by the relation,

$$\left( \frac{V}{V_0} \right)^2 = \frac{e^{2P(l-x)} + e^{-2P(l-x)} - 2 \cos 2Q(l-x)}{e^{2Pl} + e^{-2Pl} - 2 \cos 2Ql} \quad (36)$$

In the above equations  $P$  and  $Q$  stand for the functions of resistance, inductance, and capacity given above in Section 14, and  $e$  is the Napierian base.

The equation (36) enables the pressure at any point in the cable, as given by a voltmeter, to be predetermined from the pressure at the origin. The current flowing through a low-

resistance ammeter placed at any point in the cable can also be deduced from the equation (36), and its maximum value,  $I$ , is shown by Mr. Heaviside to be given by the equation,

$$I^2 = \frac{V_0^2 C p}{\sqrt{R^2 + p^2 l^2}} \frac{e^{2P(l-x)} + e^{-2P(l-x)} + 2 \cos 2Q(l-x)}{e^{2Pt} + e^{-2Pt} - 2 \cos 2Ql} \quad (37)$$

This gives us the value of the dynamometer reading at any point in the cable. It shows us that if  $l$  is great enough the current flowing through an ammeter placed at the distant end of the cable would be zero.

Suppose, next, that the distant end of the cable is open-circuited, and that an electro-motive force, of which the maximum value is  $V_0$ , acts at the home end. It is then possible to prove that under these circumstances the ratio of the maximum value,  $V$ , of the potential at any distance,  $x$ , from the origin, to the maximum value of the potential at the origin, is given by the equation,

$$\left(\frac{V}{V_0}\right)^2 = \frac{e^{2P(l-x)} + e^{-2P(l-x)} + 2 \cos 2Q(l-x)}{e^{2Pt} + e^{-2Pt} + 2 \cos 2Ql} \quad (38)$$

If  $x = l$ —that is, if we consider the potential at the far end, the numerator of the right-hand side, becomes equal to 4, and hence if the denominator is numerically less than 4 for any given cable—it indicates that a progressive rise in pressure along such cable will take place from the machine end to the free or insulated end.

In a very able paper in the *Philosophical Magazine* for January, 1887, p. 21, Mr. Heaviside has attacked the general problem of calculating the current flowing through a transformer at the far end of a concentric cable when an alternator or transformer giving a periodic electro-motive force of maximum  $V_0$  is placed at the other. The paper is not an easy one to read, and was probably still harder to write. Some of the general results are, very briefly, as follows:—If  $L_0$  and  $R_0$  are the total inductance and resistance of the transmitter, whether alternator or transformer, and  $L_1$  and  $R_1$  the same quantities for the receiver, and  $L$ ,  $R$ , and  $C$  the inductance, resistance, and capacity per unit of length of the cable, and  $V_0$  the maximum value of

the impressed electro-motive force at the transmitter, and  $C_0$  the maximum value of the current through the receiver, then

$$= 2 V_0 \frac{C p}{(R^2 + p^2 L^2)^{\frac{1}{2}}} \left[ G_1 G_0 e^{2P l} + H_1 H_0 e^{-2P l} - 2 (G_1 G_0 H_1 H_0)^{\frac{1}{2}} \cos 2 (Q l + \theta) \right]^{-\frac{1}{2}}$$

where  $G_1$ ,  $G_0$ ,  $H_1$ ,  $H_0$ , and  $\theta$  are complicated functions of the resistances and inductances of the receiver and transmitter, and which have the values,

$$G_0 = 1 + (P^2 + Q^2) \left( \frac{I_0}{I} \right)^2 + (2 P R + 2 Q p L) R_0 + (2 P L p^2 - 2 Q p R) L_0;$$

$$H_0 = 1 + (P^2 + Q^2) \left( \frac{I_0}{I} \right)^2 + (2 P R + 2 Q p L) R_0 - (2 P L p^2 - 2 Q p R) L_0;$$

and similarly for  $H_1$  and  $G_1$ .

$I_0$ ,  $I$ , and  $I_1$  are the impedances of the transmitter, cable, and receiver; and the quantities  $P$  and  $Q$  have the values given in Section 14.

The quantity  $\theta$  is an angle which is determined from the expression,

$$\tan 2 \theta = \frac{2 (A b - a B)}{A^2 + B^2 - (a^2 - b^2)},$$

where

$$A b - a B = (R_0 + R_1) (R Q - L p P) + (L_0 + L_1) p (R P + L p Q) - (R_0 I_1^2 + R_1 I_0^2) C p P - (L_0 I_1^2 + L_1 I_0^2) p^2 C Q;$$

$$A^2 + B^2 = I^2 + C^2 p^2 I_0^2 I_1^2 + 2 (R_0 R_1 - L_0 L_1 p^2) L C p^2 - 2 (R_1 L_0 + R_0 L_1) p^2 R C;$$

$$a^2 + b^2 = (P^2 + Q^2) [(R_0 + R_1)^2 + p^2 (L_0 + L_1)^2].$$

The question whether the mean pressure at the receiver is greater than the mean pressure at the sending end, or less, will in general depend upon the sign of  $\tan 2 \theta$ .

The quantities  $G_0$  and  $H_0$  become equal to unity when the inductance and resistance of the sending instrument are zero or very small.

The practical deduction which may be made from the above investigation is, that when the alternator at the sending end has very small resistance and inductance, the pressure at the receiving end of the cable will be less than the pressure at the

transmitting end, for almost any cases which occur in practice; but this is not always the case when the transmitting instrument has a sensible inductance. We may then have conditions under which there is a progressive rise of pressure from the sending to the receiving end.

The time at disposal has not permitted me to do more than sketch out the general results of such investigations as have been made on the propagation of periodic currents through conductors having sensible capacity and inductance. These investigations need to be brought into further comparison with the results of experience, in order that we may see how far they agree with observed facts, and how far they can be trusted as a guide in predicting the results of the application of known electro-motive forces to conductor systems in which the capacity and inductance can be predetermined.

The PRESIDENT: I beg to move that a hearty vote of thanks be given to Dr. Fleming for his very learned, elaborate, and valuable paper. It is proposed to discuss this paper together with one by Mr. Preece, which, in his unavoidable absence, I will ask the Secretary to read.

The following paper was then read:—

## ON SOME POINTS CONNECTED WITH MAINS FOR ELECTRIC LIGHTING.

By WILLIAM HENRY PREECE, F.R.S., Past-President.

1. I do not purpose in the following paper to describe the mechanical details of the various systems of mains adopted in different places to transfer and distribute electrical energy. These, most of us can see for ourselves by personal inspection in the various works now in progress in London, or can read about at our leisure, in the numerous papers that have been recently printed in the technical periodicals and in the Journal of our Institution. My object is to extol the "sweet simplicity" of the two-wire system, the practical security of concentric mains, and the unquestionable economy of the high-pressure

principle. At the same time, I do not want to be considered an advocate of any one particular method. Engineering works must necessarily be considered a species of compromise. Conditions vary in different localities; difficulties arise in the most unexpected quarters; different ways of surmounting obstacles occur to different minds. What is good for one place need not necessarily be good for another. The independent engineer must keep his mind open; he must master his requirements; he must give every plan his consideration, and choose his best agents, despite the tyranny of the Patent Laws and the susceptibility of inventors. He must, however, be just, and see that merit is rewarded and claims respected.

2. I do not intend to discuss overhead wires. I consider them, for high-pressure electric light circuits, an abomination, especially as it is easy to show that ultimate economy is in favour of the underground system. It is only the first capital expenditure that favours overground mains; safety, freedom from accident, ease of repair, and renewals are all the other way.

3. Broadly, the different underground systems in use may be classed into those which are *fixed*, and those which are *movable*.

As a type of the first, I will instance the Ferranti concentric mains, that are buried solidly in the roadways, embedded in asphalte in wooden troughs, and which are inaccessible except by excavation; and as an example of the second, I will take the ordinary telegraphic method of burying cast-iron pipes, with occasional boxes, in and out of which, cables new and old can be drawn at pleasure, and where the conductors are always accessible for the rapid removal of incipient faults should they develop themselves, and removable for enlargement should the system grow.

4. Again, we must divide the systems into *high* and *low pressure*, for under these two heads we have to consider not only the all-important questions of insulation and of finance, but those of security to person and to property. The line of demarcation between high and low pressure has been defined by the Board of Trade to be 300 volts; but in what follows, whenever *high pressure* is mentioned about 2,000 volts

is meant, and *low pressure* implies about 100 volts. There is one case only in this country where *extra high pressure* is used, and that is by Mr. Ferranti, whose adoption of 10,000 volts is regarded by so many with fear and horror. This voltage is, however, going to be exceeded this summer by the Exhibition authorities at Frankfort, who are going to draw 300 horse-power from the river Neckar, at Lauffen, 112 miles away, by a pressure of 27,000 volts.

5. The only limit to the use of extra high pressures, or to the voltage employed, is the efficiency, economy, and durability of insulating materials.

First and cheapest on the list is *air*, which, when dry, is the most perfect insulator known—its specific resistance being, in fact, immeasurable—which has the lowest specific inductivity, and therefore is taken as unity, but which cannot resist electric stress, and through which the sparking distance has a very poor figure of merit. The breakdown stress, or sparking distance, of dry air at ordinary atmospheric pressures may be taken to be for—

Direct currents	...	...	9,200 volts per centimetre ;
Periodic alternating currents	6,000	„	„

between a disk and point.

Air is, moreover, hygroscopic, and very variable in its qualities ; but there is no evidence that the variation of its saturation point affects its resistance or its inductivity, excepting that it tends to coat with moisture, glass, porcelain, and other insulating supports, which are essential to keep up metallic conductors in position when it is used as an insulator, if their temperature falls below that of the air. The use of air was inaugurated by Mr. Crompton in Kensington, and his example has been followed by Latimer Clark, Muirhead, & Co. in St. James's, by Professor Kennedy in Westminster, and by Professor Robinson in St. Pancras. It has also been followed in Paris and in Germany.

6. *Gutta-percha* has been used almost exclusively for submarine cables, and for telegraphic underground wires in Europe ; but owing to its scarcity and its high price it is rapidly being replaced by other materials, and, excepting in Paddington (Great

Western Railway), where it was laid down by the Telegraph Construction Company, I do not know of its use for electric light purposes. It does not stand exposure to variations of temperature or of dryness. It is durable only in water, and even if it were plentiful it is doubtful if it were practical for electric light purposes.

7. *India-rubber* is more practical. Modern improvements in vulcanisation have brought it within the scope of the engineer and the range of finance. The addition of a small quantity of sulphur, subjected to high temperature, produces a new form of rubber, which makes not only an admirable insulator, but a waterproof, homogeneous, compact, and apparently durable compound. Our experience of this latter quality is, however, limited at present. The cables of this class manufactured by the Silver-town Company have justly acquired a high reputation, though several other firms are competing with them on equal merits. The weak point is in the jointing, which, unless properly done and thoroughly vulcanised, admits into the system numerous points of danger, which, sooner or later, will occasion engineers in charge many minutes of anxiety and worry. Compound rubber cables are used exclusively by the Metropolitan Electric Supply Company and by the House-to-House Electric Light Company, for their mains distributing high-pressure alternating currents. I am assured by both companies that up to the present time not one single defect has developed itself in their mains.

8. *Bitumen* is a mineral product of a pitchy character, imported into this country principally from Trinidad. The late Lord Cochrane tried hard to introduce it as an insulating medium, but it remained for the enterprising firm of Callender to make it a real commercial commodity. When purified, refined, and vulcanised it makes, combined with some vegetable fibre like jute, a tough elastic coating, proving it to be an active competitor of compound rubber. When used in its crude state it forms a solid mass, in which the more refined "bittite" can be embedded in an immovable and apparently indestructible and unassailable bed.

9. There are various waxes—*paraffin*, *ozokerit*, and other

hydrocarbons—which have a high specific resistance and a low specific inductivity, that have been much used; but the most practical use of these materials appears to be their combination with brown paper, which has been introduced by Mr. Ferranti in his Deptford mains.

10. The refuse of crude *petroleum*—that is, petroleum after all the spirits, illuminating and lubricating oils, and waxes have been distilled or expressed—a thick, heavy black mass, liquid under high temperature, but almost unflammable, and eminently adapted to resist high temperatures—makes an admirable insulating compound of very high specific resistance and comparatively low specific inductivity. It is the material used by the Fowler-Waring Company, and destined to enjoy a profitable career.

11. *Heavy resin oil* is an admirable insulator, filling, when hot, all the pores of vegetable matter like jute and cotton immersed in it, excluding all air and moisture from the conductor, having a very high resistance and a very short sparking distance, but, above all, being viscid and movable, so that any puncture is at once filled up by its own mobility, so that any fault occasioned by piercing is instantaneously self-cured. Oil has such excellent merits that it has been proposed by Johnson & Phillips to bury in it high-pressure alternate-current transformers. Heavy oils discard water owing to their density. They are inoxidisable, and therefore practically indestructible. Many failures in the use of oil as an insulator in the past have arisen from the use of oils of too low specific gravity.

12. There is one objection to most of these mineral hydrocarbons, viz., that they require to be encased in lead; but oil has this advantage—that it can be used in iron pipes in the method adopted by Brooks. Lead covering is expensive, and it adds seriously to the weight of the cable, so as to render more difficult its handling and drawing in and out. On the other hand, lead adds materially to the life of the cable. I have in my possession samples of lead-covered wire which was buried in 1844, and in which the conductor and insulating medium are now as perfect as

when made. Lead, however, in some localities, especially in decaying vegetable matter, is itself attacked. When, however, properly protected, it ought to last for centuries. The lead used by the Romans to distribute water in Pompeii is still visible. Lead-covered cables are very largely used in America, Germany, and France, and their employment is rapidly extending in England.

13. All these materials, excepting air, are equally applicable for high and for low pressures: the thickness only has to be increased with the pressure; but we have yet to know what influence the rapid reversals of high-pressure alternating currents have upon the molecular structure of the material. The difference in the striking distance in air shows that the stresses in the two cases of direct and alternating currents are very different. Time alone will give us the experience we want as to the effect of these stresses on the texture of the dielectric used. It is this absence of experience that causes so much want of confidence in the use of extra high pressures.

14. The operation of Sir William Thomson's law, the relative economies of the two-wire, three-wire, and five-wire modes of distribution, the comparative cost of high and low pressure, have been so frequently discussed, and are so thoroughly well known, that they need no description here. Everyone will admit that as we increase the pressure driving the currents, we can diminish the size of the conductor; and although we have to increase the relative thickness of the insulating medium, the actual quantity used remains practically the same. Hence high pressure means great economy in copper. The future of electric lighting is a financial question; its practicability and efficiency are beyond doubt. The lion share of the capital necessary for the extension of electric lighting over a district is required for the mains and conductors; an important item in working expenses is in their maintenance. Hence this is now the field for the exercise of economy. If we can reduce the cost of distribution and its upkeep by 50 per cent., we should be able to supply light by electrical energy more cheaply than by gas. It is a pity that some of that redundant mental energy which is endeavouring to raise

the efficiency of dynamos and transformers from 94 per cent. to 95 per cent. could not be transferred to the question of mains, where real advancement and true economy on a considerable scale are much more required.

In Liverpool, Birmingham, and Bradford, low pressure is distributed by the original and simple two-wire parallel system. In Kensington and Westminster low pressure is distributed by the three-wire plan. The London Electric Supply Company, the Metropolitan Company, and the House-to-House Company distribute their high-pressure systems by two wires; but in Rome, where sub-stations are used, the distribution of low pressure is by three wires. In Germany distribution in some places is by five wires, and I have heard it mooted to distribute in Paris by six wires.

15. The points that determine the distribution of low pressure are fall of potential, and number of feeding points and distributing stations. We need not consider the heating of conductors. If we take care of the potentials, the waste will take care of itself.

One ton of ordinary copper of one square inch sectional area covers a length of 192.3 yards, and its resistance is  $\cdot 0046 \omega$ . Hence, a length of 220 yards weighs 1.13 tons, and its resistance is  $\cdot 00527 \omega$ . A pair of such mains will weigh 2.26 tons, and their resistance will be  $\cdot 01054 \omega$ . 1,000 amperes means, therefore, a loss of  $10\frac{1}{2}$  per cent. (and one kilowatt is wasted in every 43.4 yards), 500 amperes means a loss of  $5\frac{1}{4}$  per cent., and 237 amperes means a loss of  $2\frac{1}{2}$  per cent., in such a circuit when the full load is supposed to be concentrated at the end. If we take a fall of potential of  $2\frac{1}{2}$  per cent. in each main, which is the recognised maximum waste at full load allowed to comply well with Thomson's law, it means 474 amperes delivered at 100 volts, or 1,600 8-candle-power lamps served. In fact, we may say that two mains of one square inch copper will serve 1,600 8-candle-power lamps at 220 yards distance; and this appears to be the commercial limit of the two-wire low-pressure distribution. Of course, if we are

content to admit a greater waste, this limit of distance can be extended, and the number of lamps to be served can be increased in the same ratio. The exact waste to be allowed is an economical question governed a good deal by the conditions of the problem, financial as well as engineering. Now, if we add another 100 volts at the point of supply, we can deliver the same energy for the same number of lamps with the same loss, at 880 yards; and for every additional 100 volts delivered, we can extend the distance to which the supply can be driven under similar conditions, as the square of the unit distance, 220 yards, for to maintain the same energy the current is *diminished* in the same ratio. Thus 2,050 volts will drive the energy required for 1,600 8-candle-power lamps 50 miles if we use mains of one square inch sectional area, and are satisfied to waste only 5 per cent. of the driving energy. Professor John Hopkinson, F.R.S., showed how,\* by the use of an intermediate third wire and two dynamos, we could use 200 volts with our present lamps, and therefore we could practically diminish for the same distance the weight of copper used by one-half, or we could make the same weight of copper serve double the distance. The third wire, however, diminishes the effective economy of the plan. Its practical commercial limit appears to be about half a mile; and its disadvantages to be, more dynamos to work, and considerable complication to adjust and regulate the equilibrium of lamps on the circuit.

The five-wire system, which is in use in Germany, enables us to use 400 volts at the point of delivery; and this, again, enables us to extend our area of supply with the same weight of copper, to one mile, but with more dynamos, and with more regulation and balancing, and therefore with more complication.

There is no practical limit to the high-pressure system: 10,000 volts will drive energy from Deptford to London on two mains of one-quarter square inch section to serve 50,000 lamps; and 27,000 volts will drive 300 horse-power 112 miles, from Lauffen to Frankfort, on a conductor of .06 square inch sectional area, *i.e.*, a No. 2 copper wire.

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\* John Hopkinson, Patent No. 3576, July, 1882

16. But we have to take into consideration the difficulties of the distribution.

On the two-wire system this means one central station for each effective area having a mean radius of 220 yards. On the three-wire system this area can be extended to a mean radius of one-half mile; the five-wire system extends the mean radius to one mile; but with the high-pressure system the area is practically unlimited in extent. When the mains and feeders are at high pressure, and the distribution at low pressure by two or three wires, we must contemplate *sub-stations* serving areas having a radius not exceeding 220 yards, if the lamps be uniformly scattered over the area and our waste energy is kept low.

A transformer sub-station is a very simple and cheap structure. It may be excavated underground, or be in a cellar or in a back-yard. The instruments are stationary. They require no oiling, nor adjusting, nor handling, nor even tending. They can be buried in oil and suffer no deterioration. They are simple in construction, and should need no repair. Switches to throw them in and out of circuit may be automatic or manual, and can be operated from outside the transformer room, as is done in Rome.

17. The sub-station high-pressure system has been thoroughly worked out in Rome. Every nest of about 2,000 lamps is served from a sub-station containing a bank of 12 transformers, each of  $7\frac{1}{2}$ -kilowatt capacity. Means are provided to cut transformers in and out as the load comes on and off. The great waste of the idle transformer is thus prevented, and by banking them in this way only half the total number of transformers is required. Moreover, it enables the most efficient transformers to be used, and always at full load. It is well known that the efficiency of a transformer increases with its size. Mr. Ferranti has already reached 100 kilowatts without reaching a limit to this law. Distribution by two wires on this limited scale is of the most economical and effective kind, and there is no complication of adjustment of current or regulation of pressure. Each sub-station serves, as a rule, in Rome, one block of buildings, so that generally the high-pressure mains alone are underground.

The pressure is 2,000 volts. The distribution in Rome is by three wires—not, however, for any economical reason, but simply to allow arcs and glow-lamps to work on the same network without interference and without waste. The counter electro-motive force of the arc prevents arc lamps from being registered accurately on any of the existing meters, but the three-wire plan enables the arcs to be served separately from the glow-lamps, which can be accurately metered without any disturbance from the arcs. Arc lighting is in great request in Italy, especially for shop-lighting.

18. It is impossible to serve economically a wide and scattered district with a mean radius of over one mile by any other means than by high pressure, for we cannot multiply central stations without seriously increasing capital outlay and working expenses; unless, indeed, the density of supply be very great, as it is in some quarters in London and in some of our large provincial cities.

19. Let us assume that we have a wide and scattered district to serve, and that we are going to supply at high pressure, and to distribute by two wires from sub-stations: the problem we have to solve is, What shall be the character of the high-pressure mains and feeders connecting the central station, or stations, and the sub-stations?—shall they be separate conductors or concentric cables? We will also assume that the weight of copper and quality of insulation is the same in each case, and that their prime cost is not materially different, although, as a matter of fact, for the same distance the concentric main, if made of the same materials, costs less than the two separate conductors.

It is therefore a question of *adaptability*, *security*, and *freedom from disturbance*. To study these points, we want to know how the potentials are distributed on such conductors.

If we consider two independent conductors (Fig. 1), lying side by side in the same pipe, or conduit, connecting the two poles of the dynamo, D and D', with the transformer, C C', and we call the *go* wire the

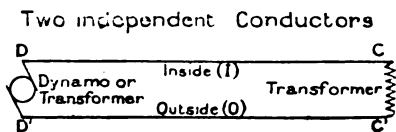
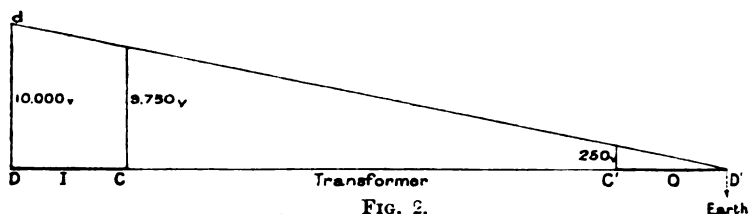


FIG. 1.

*inside* (I) and the *return* wire the *outside* (O),—then, if

we represent the effective resistances at any moment of the transformer by the straight line  $D D'$  (Fig. 2),  $D C$  can



represent the resistance of the inside conductor,  $C C'$  that of the transformer, and  $C D'$  that of the outside conductor. If the ordinate  $D d$  represents the positive pressure when at its maximum and the transformer on full load, then, if the pole  $D'$  be connected with earth, the slope of the line will indicate the simultaneous pressure at any point of the circuit, and the difference between the ordinates will give the fall of potential between any two points; that is, there is a fall of 250 volts in each conductor, and if the circuit be carrying 50 amperes there will be a waste of 12,500 watts in each conductor. If the line  $O$  were in its proper position and direction alongside  $I$ , as in Fig. 1, it would be seen that the difference of potentials between any contiguous points is everywhere 10,000 volts.

Now, if the earth be transferred from  $D'$  to the negative

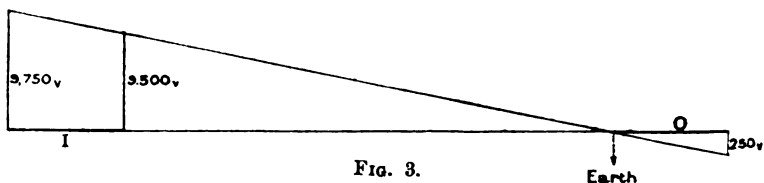


FIG. 3.

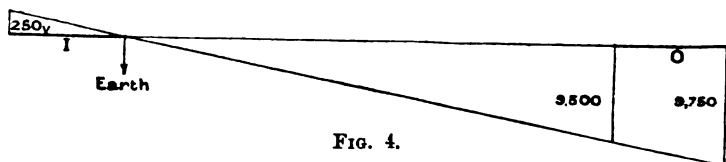


FIG. 4.

pole of the transformer,  $C'$ , the pressure at the similar phase will rearrange itself as shown in Fig. 3; and Figs. 4 and 5 give the

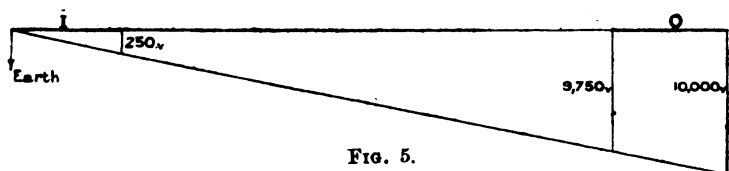


FIG. 5.

same information when the earth is consecutively fixed at the other pole of the transformer and dynamo. Fig. 6 shows the

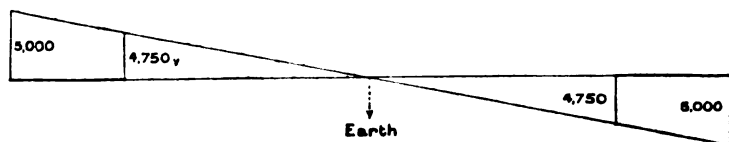


FIG. 6.

distribution when the centre of the transformer is to earth, or when there is no earth. It will be seen in each case that the fall of pressure remains the same; the difference between the potentials between the conductors remain the same: it is the sign and the difference of potential between each point and the

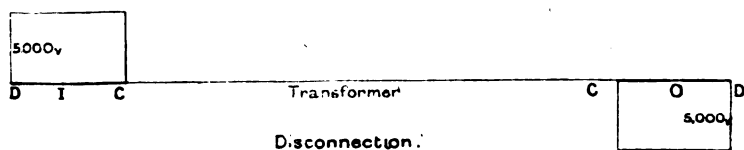
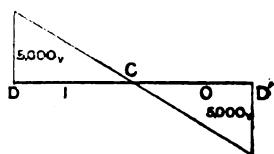


FIG. 7.

earth that has altered. Fig. 7 shows the distribution when the transformer by some accident is disconnected, and Fig. 8 when it is short-circuited.

I have not considered the variations of potential due to the rise and fall of the currents, and I have ignored the dynamo—the potentials being those at its terminals. Frequency, inductance, impedance, besides capacity and resistance, enter to complicate matters and to deprive the graphical method of that simplicity which is shown by the figures. But Dr. Fleming has exhaustively dealt with this subject analytically and in a masterly manner.



Short-circuit  
FIG. 8.

20. Now Mr. Ferranti's mains are concentric, and their capacity

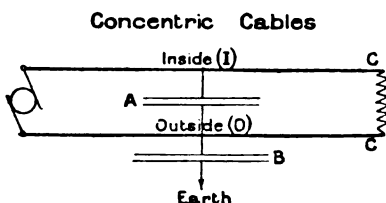


FIG. 9.

must be taken into consideration. Fig. 9 shows these capacities, that between the two conductors being .35 microfarad per mile, and that between the outer tube and earth being 3 microfarads per

mile. Figs. 10 and 11 show the distribution of pressures

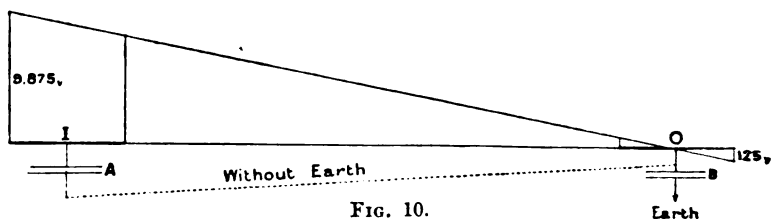


FIG. 10.

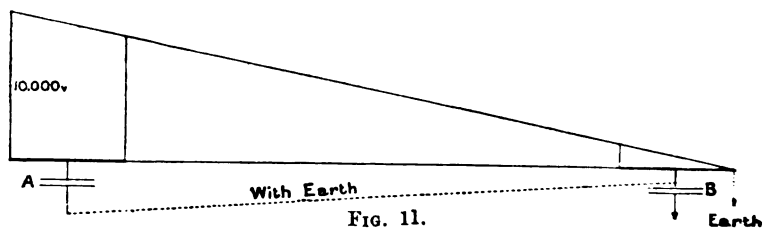


FIG. 11.

when the earth is on and off the pole of the dynamo. The remarkable fact comes out that whether the pole be to earth or not the difference of potential between the outer conductor and earth of a concentric cable is practically the same in each case. It does not exceed 250 volts in either case, and therefore the cable could be handled with impunity while the current was flowing and all connections were right, but if a fault arose—disconnection or short-circuit—then the result might be serious. A long length of perfectly insulated cable with considerable capacity is practically in the same position as though it were connected to earth. The surging in and out of the cable of the electrical quantity dependent on the capacity and the impressed electro-motive force, produces the same effect at the terminals as the periodic flow

of current through the cable when the distant end is to earth. The effect of capacity is to lower the potential, and when its magnitude is sufficient it reduces the potential of the conductor to that of the earth, for it does not allow time enough for the charge to accumulate. Hence, a concentric main, whether its outer conductor is connected to earth or not, has its potential reduced practically to that of the earth. The use of the earth is imperative to dissipate the static charge and to secure safety. A permanent and efficient earth makes the cable safe to person, and dangerous to apparatus only, which can be fully protected by fuses. Unless, however, it is most effectively applied, it may become a source of danger to person also, and of disturbance to telephones, for the existence of a variable fault might produce a shifting of potentials which would destroy the security.

It is very easy by the aid of such diagrams to study the distribution at any phase of the potential, or at any instant of flow. The capacity acts as a kind of break or damper on the currents, and its tendency is to vary the rate of rise of potential, and to flatten the curve of rise and fall of the currents. With direct and intermittent currents it acts as a fly-wheel, but with alternate currents it acts more as a spring, or as an air cushion in a hose-pipe. Its effect with ordinary frequencies is not material; but the rapid alternations of stress, and displacement in the dielectric must have a tendency to heat that material. It will be readily seen how the earth dissipates the charge at once, but it also shows that when a disconnection takes place this discharge may be very unpleasant.

21. Such being the distribution of potential on a concentric main, and also on a system of two parallel conductors, it is now desirable to compare their relative security. We have to consider, not alone what happens when everything is in order, but what happens when accidents and faults arise. Nothing is perfect—we have only before us a choice of evils. Earth is the electrician's bane, but there may be circumstances under which it may become his antidote. No one but Mr. Ferranti has had the temerity to employ the earth as his antidote, and I must

confess that I have been compelled to effect a compromise with him on the subject. His original form of cable was designed so as to place the outer tube completely to earth, but my own experiments\* and his experience have shown that this outer tube must be thoroughly well insulated to prevent the introduction of serious disturbing effects on telegraphs and telephones. In Rome, where Siemens concentric cables are largely used, but without earth, the outer insulator was so frequently pierced, that Professor Mengarini found it necessary to make the outer insulation thicker and better than the inner. The sudden stoppage of a flow of great energy from any cause must lead to effects analogous to those of momentum, and unless some buffer or cushion is provided, either in the form of a condenser or of the earth, a heaping up of pressure must occur somewhere, and sparking and rupture follow.

22. Mr. Ferranti claims for his concentric mains with the outer conductor earthed at one end, as shown by Fig. 11—

1. Absolute safety in handling by workmen, and freedom from fire risk.
2. Dispersal of the static charge accumulated on the conductors.
3. Freedom from disturbance on neighbouring wires.

I can corroborate the last point. We in the Post Office are now quite unaware whether the Deptford mains are at work or not. It was not always so. At first, starting with imperfect cables, the disturbances were very serious.

The compromise effected between the Postmaster-General, the Board of Trade, and the London Electric Supply Corporation is shown by Figs. 12, 13, and 14, which also give some of the changes that have been made.

We allow earth on the outer conductor at Deptford, and at each distributing station, but not on mains other than concentric, or on the house service excepting through a safety device.

With regard to high-pressure alternating-current systems generally, earth cannot be admitted on house wires, because with

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\* "On the Disturbances arising from the Use of Earth for Electric Lighting Purposes," March 28, 1889.—*Journal*, vol. xviii., p. 314.

its use, high insulation is imperative to reduce fire risks, and high insulation cannot always be maintained. Leakage means danger,

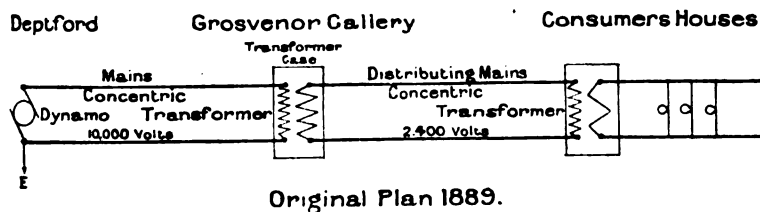


FIG. 12.

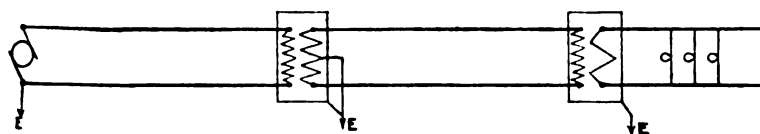


FIG. 13.

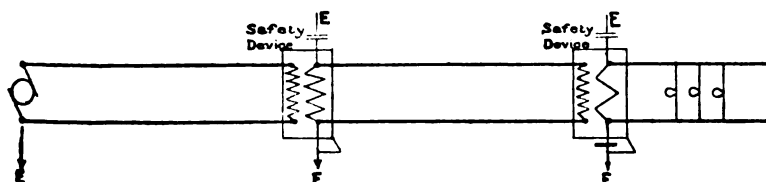


FIG. 14.

in spite of fuses and safety devices. Fire and life risks are intensified by bad earths. Good earths are very difficult to secure, and alternate currents do not like earth; that is, the resistance of earth to alternate currents is much greater than that to direct currents.

We can secure safety in houses by—

1. Good materials.
2. Good workmanship.
3. High insulation.
4. Well-designed safety devices.
5. Frequent tests.
6. Incessant personal supervision.
7. NO EARTH.

In the wiring of houses, supply companies and corporations cease to exercise responsibility. They deal with confiding customers, irresponsible contractors, untechnical and uneducated *employés*, and all sorts and conditions of men. Were it not for the Board of Trade to regulate our mains, and the Fire Insurance Companies to regulate our house wires, electric lighting in England would be as dangerous a pursuit as it appears to be in the United States.

23. The Board of Trade has forbidden the use of earth on separate conductors. This is absolutely necessary, for, except with concentric mains, the use of earth on one conductor might be exceedingly dangerous if the other conductor, by fault or otherwise, made earth. The inner conductor of a concentric main is so hermetically sealed, and so thoroughly protected by a succession of coatings, that it is well-nigh impossible for a leak to earth to be caused on it. Faults would take the form of a short-circuit, not of earth. A system to earth is one which has been placed at full cock; a permanent fault has been put on intentionally. Equilibrium has been disturbed, and it is the security of the inner conductor of the concentric main that alone makes it acceptable.

24. The chief accident that one has to fear in the high-pressure alternate-current system is the piercing of the insulating medium between the primary and the secondary conductors of the transformer, and the formation of a contact between the two. Such a fault admits the high pressure into the low-pressure system, and has been the source of nearly all the troubles that have arisen. Earthing the conductor puts a great strain on this medium. Risks of fire are increased. Additional preventive measures are necessary, and hence safety devices—the best at present in the market being Major Cardew's.

The use of safety devices which earth the conductor *at once* when the transformer is pierced, or when high pressure enters the low-pressure system, is, in my opinion, an absolutely necessary safeguard, and I never issue any specification without requiring Cardew's device, which is thoroughly effective and reliable.

25. The use of low or of high pressure should not be a

question of opinion or of objection. The supposed waste, danger, and difficulties of the one are disappearing under the able tuition of experience; while the complicated regulation and variable distribution of the other are submitting to inventive skill. It is now a question of calculation and of finance. The only variable is the number of feeding points. The determining cause and the uncertain element is the density of lamps per acre, or per mile of street. We start with this initial difficulty—that we do not know our customers, and we have to prepare for a visionary *clientèle*.

This paper has been a little discursive. Its principal object is to direct attention to the simplicity of the two-wire parallel system of distribution, to the security of the concentric main, to the devices to promote the safety of the high-pressure system, and, above all, to the necessity for regarding the whole question from the engineering maintenance point of view rather than from that of the speculative promoter.

Upon the motion of the PRESIDENT, a unanimous vote of thanks was accorded to Mr. Preece for his interesting paper, and the discussion on the two papers was postponed until the next meeting.

A ballot for new members took place, at which the following candidates were elected:—

*Students:*

Henry K. Domville.                      |      John Mackenna.

The meeting then adjourned.

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The Two Hundred and Twenty-fourth Ordinary General Meeting of the Institution was held at the Institution of Civil Engineers, 25, Great George Street, Westminster, on Thursday evening, May 14th, 1891—Professor WILLIAM CROOKES, F.R.S., President, in the Chair.

The minutes of the Ordinary General Meeting held on May, 7th, 1891, were read and approved.

The names of new candidates for election into the Institution were announced and ordered to be suspended.

The following transfer was announced as having been approved by the Council :—

From the class of Students to the class of Associates—

Ernest John Welfare.

A donation to the Library was announced as having been received since the last meeting from J. Bucknall Smith, to whom the thanks of the meeting were duly accorded.

The PRESIDENT : The order of our business this evening is to discuss, firstly, the paper by Dr. Fleming, and then, if time permits, Mr. Preece's; but before the discussion commences, there is a short paper by Mr. Crompton bearing on this subject.

The following paper was then read :—

## ON THE MOST ECONOMICAL MODE OF FEEDING A LOW-PRESSURE NETWORK.

By R. E. CROMPTON, Vice-President.

The advocates of alternating transformer (hereafter called the A.T.) system of distribution, appear at last to be in agreement with those who designed low-pressure systems, in that they admit that for the supply of towns of ordinary compactness a low-pressure network must be provided. Their practice in the future is to diverge from their practice in the past, in that they fix the transformers only at the feeding points of the

network, instead of in each of the consumers' houses. In other words, they propose to use high-pressure feeders, consisting of cables carrying a high-pressure primary current, transformed so as to supply a low-pressure network on either the two- or three-wire system, by a transformer fixed at the feeding point. Mr. Ferranti, Mr. Gordon, and other gentlemen advocate banking the transformers at these feeding points, and the use of automatic apparatus to switch their transformers in as the load requires. Others, such as Mr. Mordey, prefer to spend more money on the perfecting of a design of a single transformer at the feeding point, so as to avoid the complication of automatic apparatus. As it was evident that there is a certain length of feeder at which the low-pressure direct system fails to compete successfully in first cost and that of upkeep with the above-described high-pressure A.T. feeder, I have recently investigated the question in order to find out at what mean length of feeders the low-pressure and high-pressure systems compete on equal terms of first cost, cost of upkeep, and efficiency. The result of my investigation is to show that when a three-wire low-pressure system, distributing direct to 110-volt lamps, is compared with an A.T. system, distributed by 2,000-volt feeders, transformed to supply the same three-wire network, the average annual efficiency of the feeders in both cases being 90 per cent., the low-pressure feeder is cheaper for all distances up to 2,400 yards, each of the feeders being calculated to supply 100 kilowatts; and the annual cost for upkeep, including a sum of 5 per cent. for interest on the capital, will be about £290 per annum. These large figures lead me to compare with them the known cost of upkeep of feeders carrying the same load which are now in use in London. I find that feeders of an average length of 600 yards cost about £300 each, and for interest and upkeep £25 per annum. This comparison shows that if we increase the length of each feeder by 1,800 yards, or say one mile, it increases the supply company's annual upkeep charges by not less than £260 per annum per feeder. If we apply these figures to a company supplying a district of about a square mile, which, if it had

within it two generating stations, could feed its network with 10 short feeders, and substitute for these two central stations one external generating station one mile distant from the outside of the district, the extra cost of upkeep of the 10 long feeders will be £2,600 per annum. At a load factor of 10 such a district would sell 876,000 units per annum; therefore the above sum of £2,600, divided by this number of units, is equal to 0.72 of a penny per unit sold. Now even at present the cost per unit for coal and ground rent (which are the only two items affected by the removal of the site from a central position to a distant point) only slightly exceeds 1d. a unit, and in all probability during the present year this figure will be brought down to a sum approximately the same as the above-mentioned upkeep of the feeders, viz., 0.72, which I have shown is the actual cost of upkeep of the feeders only. This shows the absurdity of imagining that any saving can be effected by this mode of feeding a network, and it is quite evident that those who have advocated the system have never seriously looked at the matter from this point of view. In such cases where really great savings can be effected by generating the energy at a distance from the district to be supplied, as in the case of water power, Mr. Ferranti's original Deptford scheme of supplying one or two large transformer stations by currents of very high E.M.F. is in all probability the right one; but the distribution must be modified from Mr. Ferranti's original intention: that is to say, he need not transform twice, but merely supply several transformer stations by his high-pressure mains, and feed the surrounding districts by not only a low-pressure network, but also low-pressure feeders. I have not at command the figures as to the cost of mains of extreme high pressure such as Mr. Ferranti advocates; but I have no doubt that he will be able to show, even for such distances as one or two miles, a very considerable saving on the separate feeder system that I have just criticised.

Table I. shows you the details of first cost of the 600 yards low pressure, 2,400 yards low pressure, and 2,400 yards A.T. feeder with transformer.

Table I.—Comparison of Cost of 100-Kilowatt Feeders.

	A. 220-Volt Low-Pressure Feeder. Mean Annual Efficiency, 90 per cent. 450 yds. Bare Copper; 150 " Cable in Pipes; Total, 600 yards.	B. 220-Volt Low-Pressure Feeder. Mean Annual Efficiency, 90 per cent. 1,800 yds. Bare Copper; 600 " Cable in Pipes; Total, 2,400 yards.	C. 2,000-Volt A.T. Feeder, with Transformer in Pit. Mean Annual Efficiency, 90 per cent. 2,400 yards Concentric Cable, laid in part of Low-Pressure Culvert or in open.
Excavation, or extra width culvert and pipes, at 5s. per yard ...	£ 150	£ 600	£ 600
Transformer pit ... ..	...	...	60
Bare copper, at £90 per ton, laid ... ..	78	1,260	...
Low-pressure cable (250 volts), rubber insulation, laid ...	65	1,020	...
High-pressure cable (concentric), 2,000 volts, laid ...	...	...	1,300
Transformer and apparatus, including fixing ... ..	...	...	440
<b>TOTAL</b> ... ..	<b>£293</b>	<b>£2,880</b>	<b>£2,400</b>

Table II.—Cost of Upkeep of 100-Kilowatt Feeders.

	A. £ s. d. 1 10 0	B. £ s. d. 6 0 0	C. £ s. d. 6 0 0
Culvert and pipes, &c., at 1 per cent. ... ..	...	...	1 10 0
Transformer pit, " 2½ ... ..	...	...	...
Bare copper, " 1 ... ..	0 15 8	12 12 0	...
Low-pressure cable, " 8 ... ..	5 4 6	81 10 0	...
High-pressure cable, " 8 ... ..	...	...	104 0 0
Transformer and apparatus, " 10 ... ..	...	...	44 0 0
Small apparatus ... ..	2 10 0	10 0 0	10 0 0
Interest on total, 5 per cent. ... ..	14 13 0	144 0 0	120 0 0
<b>TOTAL</b> ... ..	<b>£24 13 2</b>	<b>£254 2 0</b>	<b>£285 10 0</b>

Table II. shows the details of cost of upkeep of the same three feeders. It will be noticed I have taken the cost of upkeep on excavation or concrete work below ground at 1 per cent. per annum, on the transformer pit at  $2\frac{1}{2}$  per cent. per annum, on the bare copper at 1 per cent., on both low- and high-pressure cables at 8 per cent.; although I might well claim that the cost of upkeep of the high-pressure cable would be a higher percentage on its first cost than the low. The transforming and switching apparatus I have taken at 10 per cent., and I have added a small charge for the upkeep of surface and inspection boxes. This is taken without reference to pressure, in proportion to the length laid. The cost of the low-pressure feeders has been carefully taken out in accordance with our London practice, and that of the high-pressure feeder has been taken out from estimates received from cable and transformer makers. I believe that these figures may be said to be sufficiently correct to make the comparison a fair one; at any rate, I think no one will venture to say that either the first cost or upkeep is taken at too high a figure. For the purposes of my argument they are sufficient to show that 100-kilowatt feeders of 2,400 yards long cannot be put down at less than £1 a yard, and cannot, including transformers, be maintained at less than 2s. 4d. a yard per annum.

It was agreed that Mr. Crompton's paper should be discussed at the next meeting, together with Mr. Preece's.

Mr.  
Swinburne.

Mr. J. SWINBURNE: The rise at Deptford interests me particularly, as, I believe, I was the first to explain the phenomena. The causes were difficult to investigate then, as no one knew what the phenomena were. It was generally said that the pressure rose between Deptford and London, and this was ascribed to electrical resonance, due to the capacity and self-induction of the cable, or to some property of the transformers. "Resonance" is a term taken from acoustics, and has a strict and definite meaning. On calculating the self-induction and capacity of the mains, I found that no such effect could take place. I found, however, that the effect of a capacity current on the dynamo would produce a rise all over the mains as soon as they

were coupled on. I also found there would be slight additional rise due to a change of the ratio of transformation in the step-up transformers, but not in the step-down, and that there would be no perceptible rise between Deptford and London. Mr. Kapp and Mr. Glazebrook seemed to think I did not understand my own explanation—I do not quite know why—and they wrote at some length in the *Electrician* on the subject. Mr. Kapp treated the subject with clock faces, and Mr. Glazebrook with exponential values of the sine and cosine; but neither writer added anything, interesting though their papers were. The *Electrician* then published the facts for the first time, and my explanation agreed with them entirely. Dr. Fleming's paper, however, contains far fuller details, and in particular, though I said the change in ratio of transformation would be small, it is really fairly large. This is easily accounted for. The change is due to the same magnetic leakage, or waste field, which causes the drop in transformers over and above that due to resistance. A current in a circuit with resistance only is in step with the pressure; a current in a purely inductive circuit has its rate of increase proportional to the pressure; while the current into a condenser is proportional to the rate of increase of the pressure. If the pressure varies even approximately harmonically, capacity is the antidote to self-induction. Waste field in a transformer acts precisely as if small choking coils were inserted in the primary and secondary circuits of a transformer which had no magnetic leakage. Waste field, or such coils, cause a drop at full load. If enough capacity is put in circuit, however, the drop is neutralised; and if more capacity is added, the drop is converted into a rise. I was thinking of large transformers when I said the rise would be very small, but Dr. Fleming's experiments were made with only 150-H.P. converters, and those of a type with a large drop.

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It has been urged that the effect is due to transformers, because it occurs at Deptford only when the step-up transformer is used. Whether the armature reaction or self-induction view of alternating-current machines is adopted, this does not follow. If the armature has a self-induction which makes a

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very small percentage difference in its pressure when a condenser is put on, it may give a very large percentage rise when its pressure is divided by four and its current increased fourfold. For instance, if 10 amperes condenser current raise the pressure 2 per cent., reducing the field to a quarter and taking 40 amperes condenser current gives a rise of 45·7 per cent., apart altogether from any change in the ratio of transformation.

In my explanation I spoke of armature reactions causing the rise: this is my way of putting what most people call the "self-induction" of the machine. A case which shows the insufficiency of the self-induction theory of alternators<sup>which</sup> is rather prominent just now. I refer to three-wire motors. The discussion of these is confused by people calling the laminated part which receives the alternating currents the "field magnets," and the other part, which is not always laminated, and may be, and often is, excited by a direct current, the "armature." Calling the parts according to established usage in alternating work, these motors are cases of field magnets being excited by armature reaction. I have tried a pair of Gramme alternators as dynamo and motor without exciting the motor field, and the motor starts and gives some torque. According to the self-induction theory, it cannot start or give torque. The rotary field theory is, of course, also good in a way; but there are some forms of motor which neither it nor the self-induction theory can explain, and the nomenclature and point of view are confusing. Take two Gramme alternators. Each consists of a star-shaped mass, which is separately excited, known till lately as the "field magnets." This star revolves inside a laminated ring with coils, which has been till recently called an "armature." Imagine the two machines running as dynamo and motor, and the excitation of the motor reduced to nothing. The motor still runs, but the star-shaped part must now be called the "armature," though in the generator it is still called the "field magnet." If mechanical power is applied to the motor, it will become a generator, and the other machine may have its belt removed and run as a motor; which are then the armatures, and which the field magnets, and where the rotary field explanation comes in, it is now a little difficult

to say. These actions are impossible on the self-induction theory; but they are all quite simple when treated as armature reactions. Mr. Swinburne.

I think Dr. Fleming has misunderstood the action of a transformer in altering its ratio. He deals with the self-induction of the primary. If you talk of self-induction at all,  $L_1$ ,  $L_2$ , and  $M$  must be taken into account. If  $L_1 L_2 = M^2$ , there would be no change of ratio with condensers, and no inductive "drop" on ordinary loads. Oddly enough, Dr. Hopkinson accused me of making this very mistake. I pleaded "Not guilty," but fear Dr. Fleming will not be able to clear himself.

Professor AYRTON: Dr. Fleming's paper is of great interest to me, because it confirms a number of points which were brought forward in a paper on "Alternate-Current Interference," read by Dr. Sumpner and myself, two months ago, at the Physical Society. I need not say very much about the first few pages of Dr. Fleming's paper, as they are chiefly concerned with elementary mathematics. I do not quite realise why Dr. Fleming gave all these equations, seeing that they are fairly well known. Professor Ayrton.

Dr. Fleming comes to the conclusion that electric resonance is confined within a very narrow range, and the reason that he is led to this unusual result is because he is dealing with a most unusual coil, viz., one possessing a resistance of only 10 ohms and a self-induction of 200 henries—that is, a coil in which the current would take 20 seconds after the application of the E.M.F. to rise to about six-tenths of its maximum value.

Sometimes in lectures to my students I have shown the delay in the rise of a current in a coil in consequence of self-induction, but I have had to content myself with a time constant of a second or two. What a delightful coil for lecture purposes would be the ideal one which Dr. Fleming treats as an actuality, and in which a current is to be seen creeping up for 20 seconds to only three-fifths of its steady value! In actual cases, one of which is seen on pages 373 and 374 of Dr. Fleming's paper, resonance occurs over a wide range of capacity, and not over a "narrow range," as in Dr. Fleming's imaginary case; further, we cannot have the

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P.D. at the condenser terminals ten times that at the dynamo terminals (Fig. 1), but only three or four times.

We now come to those experiments that were carried out by Messrs. Siemens. No doubt Mr. Swinburne will say the result is due to the waste field; but, at any rate, they show that the rise in the P.D. at the terminals of the secondary coil of the transformer is greater than the rise in the primary of the transformer when capacity is added to the secondary circuit. In other words, we see that for ordinary transformers the change ratio can be considerably altered by applying condensers to the secondary circuit. In the paper to which I have referred as having been given by Dr. Sumpner and myself to the Physical Society, we stated the results of experiments proving that the change ratio of a transformer could be increased by as much as 11 per cent. with a very small current taken from the secondary coil. That is to say, not merely were the volts in the primary raised, and the volts in the secondary raised, but the volts in the secondary were so much more raised than the volts in the primary that there was an increase in the change ratio of 11 per cent.; and that result was obtained when using a transformer which merely transformed up in the proportion of 1 to 2, the actual number of coils being 24 and 48. Even with equal transformation, transforming from 1 to 1, and with a small current in the secondary circuit, our experiments showed that while, on the application of a certain capacity to the secondary circuit, the P.D. at the terminals of the primary rose 5·9 per cent., that at the terminals of the secondary rose 10·3 per cent.; so that there was an increase of 4·4 per cent. in the change ratio. So also with transforming down there was an increase in the change ratio.

The general results published by us in the paper, to which Dr. Fleming makes no reference, were, to quote from the somewhat long abstract of this paper which has already appeared in all the technical journals—

- “1. That whether the transformation be up or down, the  
“percentage rise in the secondary is greater than that  
“in the primary.
- “2. That these percentage rises diminish as the secondary  
“current increases.

"3. That they increase with the ratio of transformation.

"4. That the rise in the secondary may be considerable  
"without that in the primary being appreciable.

"5. That the rise in the secondary still persists even when  
"large currents are flowing."

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The maximum increase we obtained in the change ratio was 11 per cent., while that obtained by Messrs. Siemens in the experiments referred to by Dr. Fleming was 32 per cent.; but then the transformer employed by them was transforming up in the ratio of 1 to 40, whereas ours was transforming up only in the ratio of 1 to 2.

Dr. Fleming obtains an equation, No. 31, to which I have no objection, except as to the way in which it is written. I would suggest a modification in the way of writing that equation which makes it very much simpler for ordinary people to understand. On the left-hand side we have current, whereas on the right hand we find capacity into frequency into P.D. divided by the square root of a simple number. If, however, we divide numerator and denominator by  $Cp$ , then the right-hand side of the equation becomes

$$\frac{V_0 \sin (pt - \phi)}{\sqrt{\left(\frac{1}{Cp} - Lp\right)^2 + R^2}}.$$

Now  $\frac{1}{Cp}$ ,  $Lp$ , and  $R$  are each quantities of the order of resistance, and can be measured in ohms; so that this expression, as now written, is a P.D. divided by a resistance which is obviously a current.

Later on in the paper, on pages 399 and 400, Dr. Fleming sees himself the importance of writing the expression in the form I have just given; but he does not observe that when so written a complete proof can (as Dr. Sumpner and I pointed out in our Physical Society paper) be at once obtained without using differential equations of the first and second order, and without the necessity of the steps involved in the "easy transformation" necessary to convert Dr. Fleming's equation No. 28 into his equation No. 29.

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Dr. Fleming says quite rightly, with reference to his reasoning on page 400: "This is not to be considered as a demonstration of 'the equation, but only,' &c. By starting, however, with the assumption that the current is a sine function of the time, instead of with the assumption that the P.D. is a sine function of the time, it is possible to give a very easy proof of the complete equation for the current in a circuit possessing resistance, self-induction, and capacity. And, as will be seen, this complete proof occupies no more space than the lines of reasoning employed by Dr. Fleming on pages 399 and 400 to obtain merely an indication of the nature of the expression for the current. Indeed, even when the academic method, adopted by Dr. Fleming on page 364, be followed of starting with the assumption that the P.D. is a sine function of the time, a further assumption has still to be made that the current is a sine function of the time in order to enable the differential equation of the second degree to be integrated. Hence we had better start with the second assumption only, and dispense altogether with the differential equations. Let, therefore, the current,

$$A = A_0 \sin p t;$$

then, if  $v$  be the instantaneous P.D. at the terminals of a circuit including a resistance  $r$ , and self-induction  $L$ , and a condenser of capacity  $C$ , we have,

$$v = v_1 + v_2,$$

where  $v_1$  is the P.D. between the terminals of the inductive resistance, and  $v_2$  the P.D. between the terminals of the condenser. Also,

$$\begin{aligned} v_1 &= r A + L \frac{d A}{d t}; \\ \text{and } v_2 &= \frac{1}{C} \int A d t \\ &= -\frac{A_0}{C p} \cos p t; \end{aligned}$$

$$\therefore v = r A_0 \sin p t + \left( L p - \frac{1}{C p} \right) A_0 \cos p t.$$

Hence  $v$  is obtained by projecting on a line,  $O Y$  (at right angles to the line  $O X$ , from which angles are reckoned), the hypotenuse,  $O Z$ , of a right-angled triangle having  $r A_0$  for one

side and  $\left(L p - \frac{1}{C p}\right) A_0$  for the other. Consequently,  $v_0$ , the maximum value of  $v$ , is equal to

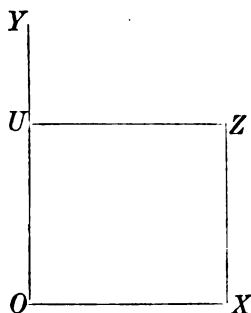
$$\sqrt{r^2 A_0^2 + \left(L p - \frac{1}{C p}\right)^2 A_0^2};$$

or

$$A_0 = \frac{v_0}{\sqrt{r^2 + \left(L p - \frac{1}{C p}\right)^2}};$$

$$\therefore A = \frac{v_0}{\sqrt{r^2 + \left(L p - \frac{1}{C p}\right)^2}} \sin p t.$$

This reasoning also furnishes at once a proof of Major Cardew's figure given on page 381 of Dr. Fleming's paper, and shows that the result of Major Cardew's figure can be obtained in a more simple way; for, instead of drawing the slanting lines as seen on page 381, we can proceed as follows:—Let  $O X$  equal  $r A_0$  and  $O Y$  equal  $L p$  or  $\frac{1}{C p}$ , whichever be the greater (Major Cardew takes the case of  $\frac{1}{C p}$  being the greater).



From  $O Y$  deduct  $Y U$ , equal to the less of the two, in this case  $L p$ , so that  $O U$  equals  $\frac{1}{C p} - L p$ : then, from what has been said above, it is clear that  $O Z$ , the diagonal of the rectangle  $O X Z U$ , represents  $v_0$ , the maximum value of  $v$ , the impressed E.M.F.

At the bottom of page 379 Dr. Fleming gives the results of some experiments made by Major Cardew to determine how nearly

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the currents sent through a condenser by an alternating current agreed with the result obtained from the following formula—the current equals the capacity of the condenser multiplied by  $p$  times the P.D. between the condenser coatings—and Major Cardew found that the actual current exceeded the calculated one in all the cases quoted by Dr. Fleming. But in the paper read before the Physical Society, to which I have more than once referred in these remarks, Dr. Sumpner and I give the results of similar experiments made by us with alternating currents and condensers; and we found, on calculating the capacity from the measured values of the current, of  $p$ , and of the P.D., that the values obtained by the formula given above, agreed wonderfully well with the capacities of the condensers determined absolutely in the well-known way by means of a ballistic galvanometer. For example, a condenser whose capacity, determined by the ballistic galvanometer, was 39.2 microfarads, appeared to have a capacity of 39.2 and 39.3 from two experiments with alternating currents.

I conclude, therefore, that all the values of the observed current in Dr. Fleming's table being greater than the calculated, must have arisen either from an error in measuring the capacity of the condenser for direct charges, or from the condenser itself being a faulty one, or from some error in the tests made with alternating currents.

The actual experiments made on the Ferranti mains, the results of which are quoted in Dr. Fleming's paper, are of great interest, and I much wish that Dr. Fleming had added one most important measurement in each case, and that is, what was the P.D. at the dynamo terminals *before* the mains were attached. The tables give us the P.D. at the dynamo terminals, at the Deptford end, at the far end of the trunk mains, &c., but they give us no information for determining what was the actual rise at the dynamo terminals produced by attaching the mains.

The numbers given in Dr. Fleming's table entirely confirm the experimental results described by Dr. Sumpner and myself to the Physical Society on March 6th, and which our students had obtained by experimenting on an artificial Deptford main.

These experiments were made with various frequencies, with various loads on the tertiary circuit, with various capacities attached to the secondary circuit, and with the transformer at the transmitting end of the trunk main transforming up, or equally, or down. Our paper contains tables showing the rise of P.D. at the terminals of what corresponded with the primary coil of the Deptford transformer; at the terminals of what corresponded with the Deptford end of the secondary circuit, or trunk mains; the rise of P.D. at the London end of these mains; and also between the terminals of the tertiary coil, or what corresponded with the London street mains.

Professor  
Ayrton.

A great deal of discussion has occurred from time to time as to what exactly occurred with the Ferranti mains, and I am happy to find that the experiments now quoted by Dr. Fleming bear out what I stated some months ago at the Physical Society—viz., that there was something like 20 per cent. greater P.D. in London than there was between the dynamo terminals at Deptford when the mains were not heavily loaded. In our paper of March 6th we pointed out that this result was fully confirmed by our experiments on the artificial Deptford main in the laboratory of the Central Institution. Mr. Swinburne was of opinion that, as our experiments were made on small transformers, the results we obtained would not be applicable to large transformers such as were actually used at Deptford; but it appears that Mr. Swinburne was not quite right in his deduction, seeing that it appears that a marked rise in the change ratio of the transformers at Deptford is actually produced by the capacity of the mains even when some hundreds of horsepower are being transmitted to London.

People may have stated that there was an actual rise of P.D. in the trunk mains themselves, but certainly I never stated that any such rise had been observed. I should like, however, to take this opportunity of correcting a mistake that I did make when sending a short note to the *Electrician* some months ago. While endeavouring somewhat hastily to convert the plan of the connections that accompanied one of the earliest of the reports of my students on this subject, and which showed all the wires and

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various measuring instruments in the exact position in which they existed in the experiments, into a diagrammatic sketch for the *Electrician*, by mistake I drew the figure as if my students' experiments had shown that there was a rise of pressure in the artificial mains themselves. This error in the figure was due to my own fault; but, when I found it out some weeks afterwards, I was led to consider whether it was possible for there to be an actual rise of P.D. in the Deptford mains themselves, and I came to the conclusion—which I now understand Mr. Heaviside had previously arrived at—that there might be such a rise, but that it would be small. And on examining Dr. Fleming's numbers it will be noticed that in *no* case when the load is small, is the P.D. at the London end less than the P.D. at the Deptford end, while in many cases it is greater. Probably the accuracy of the voltmeters employed was not sufficient to allow any important deduction to be drawn from this fact, but it is worth while noticing that the tabulated results do not disprove the conclusion that for small load there may be an actual rise in P.D. as we proceed from the Deptford to the London end of the mains.

Mr.  
Evershed.

MR. S. EVERSLED: I hardly think Professor Ayrton and Mr. Swinburne have sufficiently expressed the feeling of gratitude which I am sure is the unanimous sentiment towards Dr. Fleming for giving us the first really clear account of what was happening between London and Deptford. Mr. Swinburne has attempted a clear explanation of the condenser effect, but I notice that when he thinks he is extremely clear he is more than ever confusing. He calls it now a "storm in a teacup;" evidently Dr. Fleming has found a great deal in the teacup. It is difficult to understand the reason for the general rise which takes place, and the alteration in the change ratio of the transformer, unless you separate the two effects and consider them as distinct phenomena. I will first of all deal with the general rise in pressure, so that we do not have to take into account what is going on in the transformer itself.

In the figure is shown an alternator, A, feeding the primary coil, P, of a transformer whose secondary, S, is coupled to mains having electrostatic capacity represented by the condenser K.

K might be put on the primary mains, and the transformer might be absent, for any effect the particular arrangement indicated in the figure has on the general rise in pressure.

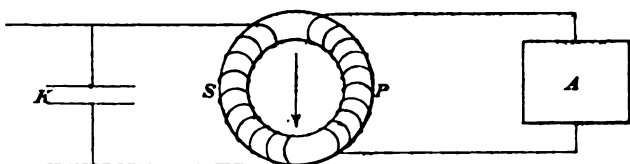
The current taken by the condenser is, as is now well understood, a quarter-period ahead of the E.M.F. wave in the mains, and consequently very nearly a quarter-period ahead of the E.M.F. impressed by the alternator. Remember that the main, or working, current in the armature coils of the alternator is cophasal, or nearly so, with the E.M.F., and consequently its maximum value occurs when the average turn of wire on each coil is half-way across the corresponding pole-piece. Evidently then the condenser current, reaching its maximum a quarter-period earlier, found the coil embracing the whole induction from the pole-piece, and so had the best chance afforded it to strengthen the excitation of the fields and cause the observed increase in the pressure. This effect was clearly explained in a recent paper read before the Institution by Mr. Swinburne.

Now we come to the second phenomenon. Why should the rise be proportionately greater on the secondary side than on the primary side of the transformer? Suppose the turns on P are equal in number to those on S. I need hardly observe that this supposition makes no difference, and simplifies the problem very much without introducing any new complication. Further suppose A maintains 2,000 volts on P, there will be about 2,000 volts on S, provided K is switched off. So long as you get 2,000 volts on P and on S, you know there is a definite induction change going on in the core, and that induction change is produced by an exceedingly small current from A, which is called the "exciting current." When we allow S to give any current, P will receive an exactly equal current from A, in addition to the exciting current. But now imagine we load up the transformer until what we may conveniently call the "load currents" are large. Recollect they are in opposite directions in P and S, and tend to neutralise each other's magnetising effects. If P and S were wound side by side all round the ring, they *would* so neutralise each other; but I have purposely drawn a badly designed transformer, and we see that at the instant that

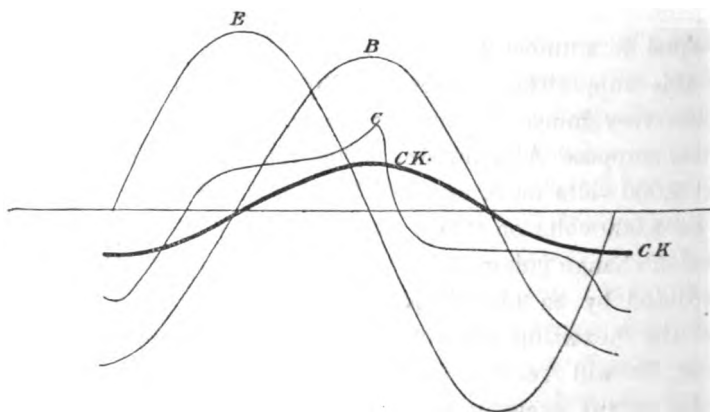
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the primary load current is at its maximum, for example, the secondary load current is so also; and these currents try to magnetise the ring in opposite directions, and they succeed to this extent—that they force some induction through the air along the path indicated by the arrow. Clearly, the induction in S will be diminished by just so much as leaks through the air in this way, and hence a bad transformer, and to some extent every transformer, gives less volts on S than on P when run loaded. That is what practical men mean when they speak of the drop in volts due to leakage induction.



*Condenser, Transformer, and Alternator  
The arrow indicates path of leakage induction*



*Transformer diagram*

*E—EMF in primary coil.*

*B—Induction in core.*

*C—Exciting current in primary coil.*

*CK—Condenser current in secondary coil*

Now let the condenser be switched on, so that in addition to the load currents we have a current flowing in and out of the condenser a quarter-period in front of the main E.M.F. In the

diagram, if E represents the E.M.F. wave on the terminals of P, <sup>Mr. Evershed.</sup> the induction wave in the iron ring will be as shown in curve B, and the exciting current will be roughly as indicated by C. The thick line, C K, represents the condenser current flowing in quadrature with E. Observe that this current comes at precisely the right time to increase the magnetising force on that part of the ring on which S is wound, or to put it in other words, to decrease the demagnetising effect of the load current in S, and so diminish the drop in volts. In the case under discussion the capacity of the Ferranti mains is so great, and the leakage induction in the transformers so considerable, that the normal drop is not only diminished, but completely swamped by the effect of the large "condenser" current. Now you will understand why I chose a bad transformer to illustrate my explanation. The whole effect is due to the leakage induction. If the transformer were so perfectly designed that the magnetising effects of P and S were identically equal and opposite there would be no leakage induction, and consequently nothing whatever to cause an alteration in the change ratio of the transformer. For reasons of safety transformer coils must be wound more or less apart from each other, so that there is always some magnetic leakage, and possibly in the Deptford transformers there is an unusually large amount.

It is worth noting that a condenser of suitable capacity coupled to the secondary terminals of a transformer enables us to do away with the drop altogether, so that the secondary E.M.F. would be absolutely constant at all loads. Or a transformer feeding a network might be made to compound up for the drop in the network in addition to its own drop. Only two or three years ago it was considered impossible to make transformers which would compound up at all, and now we seem to have stumbled on a way of solving the apparently impossible problem.

Mr. SWINBURNE: May I suggest that that increased current is <sup>Mr. Swinburne.</sup> occasionally due to what is sometimes called "soaking in"? I have found very excessive currents due to this phenomenon in condenser work. I do not know how far soaking in occurs in the

Mr.  
Swinburne

Deptford mains. I have found that the dielectric used absorbs a great deal of power, and takes an excessive current when thin—that is, under greater stresses than the London Supply Company use.

Professor AYRTON: That is just what I want to elicit from Dr. Fleming, and get some information about it.

The PRESIDENT: I would remind you that if anyone wishes to offer any observations on Major Cardew's paper he may do so, as it was originally intended that the two should be discussed together.

Mr. Adden-  
brooke.

Mr. G. L. ADDENBROOKE: There is one little point while the question of soaking in is before us. That is, we are all very apt to talk about the resistance of cables as so many ohms—megohms, rather. Those who are in the habit of testing cables know perfectly well that the resistance of cables varies in accordance with several circumstances. That is to say, gutta-percha and india-rubber cables continually increase their insulation resistance for a long time after they are made; so that, within certain limits, almost any insulation you like may be obtained by waiting until the cable has arrived at that point. Further, the insulation differs in accordance with the time the current has been on, as it is measured in the ordinary way, and therefore in giving the results of measurements it is always desirable to state the number of minutes the current has been applied. Of course in alternating work you do not apply it for a minute—only for a very small fraction of a second—and it is possible the insulation for alternating currents may differ very much from that for continuous. I pointed out this in some papers in the *Electrical Review* three and a half years ago. It seemed to me then that very possibly the insulation of a cable for alternating currents might be greater than for continuous. I do not know whether there are any experiments which absolutely prove this, or otherwise.

I should be very glad to hear some particulars about soaking-in effects in cables covered with such materials as paper. There is no doubt such cables will play a great part in the future of electrical distribution, and therefore anything that can be said of a practical nature on the subject will be gladly welcomed.

The PRESIDENT: As Dr. Fleming would prefer to postpone until our next meeting his reply to the remarks on his paper, after he has had an opportunity of referring to his notes of the discussion, some little time is still left to us, and any gentlemen who desire to offer remarks on Mr. Preece's paper are therefore at liberty to do so. The President.

Mr. SWINBURNE: I have only a few remarks to offer on Mr. Preece's paper, but I also am specially interested in the disturbance of telephone circuits, to the possibility of which I referred at the Electric Light Inquiry in 1889. I do not think Mr. Preece has discussed the point I then urged—namely, that if the outer copper tubes of the Deptford mains are earthed at Deptford, there will be an earth current of a quarter of an ampere or so at full load. Though the mains may now be earthed in this way, the trouble will not come till they are heavily loaded, as it depends on the loss by resistance along the outer copper tubes. I do not know how far an earth current of a quarter of an ampere will affect telephones. When there are a number of mains on at full load with a fraction of an ampere each, serious troubles may arise. Mr. Swinburne.

There is considerable difference of opinion among engineers as to earthing the secondary of transformers. Mr. Preece says it leads the electricity into temptation. The only way to lead electricity into temptation is to give it a high pressure. If the middle of the secondary is not earthed, the insulation on either side may, in case of the minutest leak on the other side, have the whole 100 volts on it. If it is earthed in the middle, it can have only 50 volts. If anything automatic is to be used, the best thing is a cut-out in an earth circuit from the middle of the secondary, so that if there is a current the transformer is cut off from the primary at once. Such an arrangement is secure, not only against leaks from primary to secondary, but also against danger to life or from fire due to accidental contacts or earths on the secondary circuit.

The PRESIDENT: The time has now arrived for closing the proceedings of the evening, and it is proposed at the next meeting to take, probably a continuation of this discussion before asking Dr. Fleming to reply, and then the discussion on Mr. Preece's and The President.

The  
President.

Mr. Crompton's papers. I hope the great interest, and, may I say, the liveliness, of the discussion this evening may foretell a very interesting one at our next meeting.

A ballot took place, at which the following candidates were elected:—

*Associates:*

Herbert Dudley Barlow.  
James Harold Hart.  
Leonard J. Healing.  
Hector Douglas Munro.

William Alexander Valentine.  
George C. Vaughan.  
*Lieutenant* Bruce Williams,  
R.E.

*Students:*

John E. Addyman.  
Charles Procter Banham.  
William P. Hamlyn.

F. J. Howitt.  
Edward A. Medley.  
James C. Shields.

The meeting then adjourned.

The Two Hundred and Twenty-fifth Ordinary General Meeting of the Institution was held at the Institution of Civil Engineers, 25, Great George Street, Westminster, on Thursday evening, May 21st, 1891—Mr. WILLIAM CROOKES, F.R.S., President, in the Chair.

The minutes of the Ordinary General Meeting held on May 14th, 1891, were read and approved.

The names of new candidates for election into the Institution were announced, and, this being the last meeting before the recess, it was agreed that the candidates should be balloted for that evening.

The following transfer was announced as having been approved by the Council:—

From the class of Associates to the class of Members—

Professor C. A. Carus-Wilson.

The PRESIDENT: I believe, before Dr. Fleming makes his reply, Major Cardew wishes to say a few words on the subject.

Major CARDEW: The remarks I wish to make have reference to the table in Dr. Fleming's paper stating the results of measuring capacities with alternating currents. The condensers whose capacities were measured were some kindly lent by Dr. Muirhead; but they were plates that had been condemned, and it is possible that there may have been a certain amount of leakage. I therefore asked Dr. Muirhead if he would lend me plates that he could rely upon of the same capacity—approximately a microfarad. Testing them in the same way with alternating currents, and taking the speed very carefully, I found still a greater apparent capacity than what Dr. Muir-

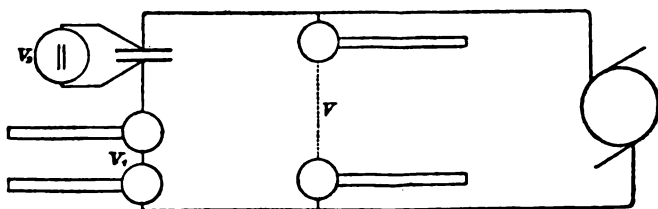
Major  
Cardew.

head's measurement gave. I understand that Dr. Muirhead measured his capacity by the method of rapid charge and discharge. In that case, it seems to me there might be a difference due to the fact that in this method the same plate of the condenser is always charged in the same way, either positively or negatively, while with the alternating current it is charged alternately positively and negatively. The two methods would therefore, I think, give results differing by the proportion of the residual charge remaining throughout the charge and discharge method, but eliminated by the method of reversal of charge. I was also able to test the amount of power taken by these one-microfarad condensers, using Professor Ayrton's formula which he has lately given us, and which comes in very usefully for the purpose; and we found with a given condenser, with about 650 volts on it, the power absorbed was only about 5 watts. The condenser did not appreciably heat, though on a very long-continued run. -

I have not the exact results with me, but I will communicate them so that they can be published with the paper.

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*The following table gives the experiments on condensers with alternating currents alluded to above. The readings were all very steady, and the speed carefully taken. The difference in the results on the two days may probably be due to a difference in temperature, which was, unfortunately, not taken.*



$V$  was measured by Cardew voltmeters.

$V_1$  was measured by two Cardew voltmeters in series, of which the joint resistance,  $R$ , was measured independently for the different values of  $V_1$ .

$V_2$  was measured by a Thomson multicellular electrostatic Major Cardew. voltmeter.

$$K, \text{ the capacity, } = \frac{C}{p V_2} = \frac{\frac{V_1}{R}}{2 \pi n V_2} = \frac{V_1}{2 \pi n V_2 R}.$$

$$W, \text{ the watts on the condenser, } = \frac{1}{2 R} (V^2 - V_1^2 - V_2^2).$$

The condenser used had dielectric of paraffin paper, and was of capacity, according to Dr. Muirhead, of 0.961 microfarad.

1891.	V. Volts.	$V_1$ . Volts.	$V_2$ . Volts.	2 R. Ohms.	N. ~.	K. Microfarads.	W. Watts.
May 11 ...	658	210.8	623	1,950	54.8	1.004	0.2
	665	216.7	626	1,952	55.8	1.016	1.5
	668	209.5	636	1,949	53.1	1.011	-1.0
	654	211.0	613	1,950	55.0	1.021	3.75
	764.5	285.5	706	1,981	63.5	1.024	2.4
	748	276.0	694	1,975	64.1	0.9982	0.8
	748	273.0	691	1,974	63.5	1.003	3.7
May 28 ...	488	218.5	429	1,952	81.7	0.9970	3.26
	490	219.0	429	1,952	82.5	0.9881	4.15
	485	217.5	424.4	1,952	82.5	0.9928	4.0
	676.8	219.0	633	1,952	55.75	0.9915	4.82

Dr. SUMPNER: Dr. Fleming has given us a great number of Dr. Sumpner. formulæ, and I do not dispute their correctness in any respect; but I wish to point out with reference to them, and also to all formulæ relating to alternate currents, that if one substitutes for the values of the resistance, R, and self-induction, L, the values which seem the most natural ones to put, the results obtained are generally quite at variance with those actually obtained by experiments. It was shown by Clerk-Maxwell long ago that if a transformer, or induction coil, as it was then called, were subjected to alternating currents, the effect of allowing a current to flow in the secondary coil was the same as if the resistance of the primary coil had been increased and its self-induction diminished. That statement was not intended to imply that the resistance of the copper coil had actually altered, but it meant that if, instead of the transformer, you had placed a

Dr.  
Sumner.

conductor having a greater resistance and less self-induction, you would get the same effect. The sort of error you are liable to in assuming that the value of  $R$  is the value you obtain by measurement with the Wheatstone bridge is very easily exemplified by giving two expressions for the difference of phase between the volts and amperes with alternating currents. For instance, a well-known formula for the difference of phase between the volts and amperes is this—

$$\cos \theta = \frac{R}{I},$$

where  $\theta$  is the angular difference of phase between the volts and the amperes,  $R$  is the resistance, and  $I$  the impedance of the coil. As the impedance is the ratio of the volts,  $V$ , to the amperes,  $A$ , or  $\frac{V}{A}$ , we have,

$$\cos \theta = \frac{R A}{V}.$$

That is the nature of the expression for the cosine of angle of lag between the volts and amperes. But if for that formula you put the value of  $R$  obtained by the Wheatstone bridge, you may get an altogether wrong result. Another formula for the angle of lag is obtained from the expression for the rate at which energy is absorbed by the coil; for if  $W$  watts are absorbed, we have,

$$W = V A \cos \theta.$$

The correct value of  $R$  to put in the formulæ must therefore be such that

$$W = V A \cos \theta = V A \times \frac{R A}{V} = R A^2;$$

that is to say, the rate at which energy is absorbed in the coil is obtained by multiplying the square of the amperes by the resistance,  $R$ . But this, as is well known, is not what you get if you substitute for  $R$  the value as measured by the Wheatstone bridge, for this only corresponds with the heating of the metallic conductor; while, if you are considering a coil with an iron core, owing to hysteresis or Foucault currents, or perhaps induced currents in a secondary coil, the watts absorbed

by the coil very much exceed those obtained by multiplying the square of the current by the resistance. The value of  $R$ , therefore, is that resistance which would absorb in heat as much power with the current actually flowing as the coil or transformer which is being considered; and it thus includes, not only the resistance of the coil, but, in addition, the resistances corresponding with Foucault currents, with loss of energy in hysteresis, and with the power transferred to any secondary coil which may be in the neighbourhood.

Moreover, the value of  $L$  used in these equations and formulæ must, of course, be obtained by alternating-current experiments, otherwise you get wrong results; and *any* alternating-current method of measuring self-induction really involves, in some way or other, the use of the formula,

$$\frac{V^2}{A^2} = I^2 = R^2 + L^2 p^2;$$

or,

$$L p = \sqrt{\frac{V^2}{A^2} - R^2}.$$

Therefore, to get  $L$ , you have to first find the value of  $R$ ; and if you use the value as ascertained by the Wheatstone bridge, you get an altogether wrong value for  $L$ . If you make  $R$  too small, you get  $L$  too large, and the value of  $\frac{L}{R}$  is estimated still higher. The differences you get in practice are not merely such as would lead to errors of 1 or 2 per cent., for you often arrive at numerical conclusions which are a thousand times too great, so that the matter is not one of small importance.

Dr. Fleming has given in his paper an example of a coil having a resistance of 10 ohms and a coefficient of self-induction 200 henries, and states that such values can be realised in practice. When I first saw the number representing the self-induction, I thought that owing to some printer's error the decimal point had been shifted three or four places to the right, for I could not believe that a coil could act with alternate currents so as to have so large a time constant. It may be possible if  $R$  is considered simply as the copper resistance of the coil, and the value of  $L$  determined so as to account for the impedance; but

Dr.  
Sumpner.

it is quite wrong to substitute such values for  $R$  and  $L$  in the expression for the difference of phase, or in any other of the formulæ.

Dr. Fleming has given sufficient particulars of the Ferranti transformers in his paper to enable us to calculate the time constant for them. He has given the resistance of the primary of the transformers as 3.5 ohms. He does not actually give the current of the primary coil when the secondary is open, but one can deduce it from the currents he gives through the condenser. If you refer to the table of observations given by Dr. Fleming, you will find that in one case the current measured at Deptford, when no load was on the transformers, was 11 amperes; but if you refer to the numbers he gives for the current of the condenser for the same voltage and frequency, you will find it is 14 amperes. The main current is, therefore, less than the current in the condenser; and the reason is that the primary coil of the transformer takes a certain amount of current which is in approximately opposite phase to the current in the condenser, and instead of adding to it, it really subtracts. The current in the primary of the transformer must be at least as great as the difference between 14 amperes and 11 amperes if Dr. Fleming's numbers—which, however, do not pretend to be very accurate—can be taken as correct. Suppose, therefore, that the current in the transformer is about 5 amperes. If the current is less than this, the value of  $R$  will be all the greater, since  $A^2 R$  is a constant, and is equal to the power in watts absorbed by the transformer. The impedance of the transformer, being the voltage divided by the current, is  $\frac{10,000}{5}$ , or 2,000 ohms, and if you assume that  $R$  is the resistance obtained by the Wheatstone bridge—that is to say, 3.5 ohms—you get  $L p$  practically equal to the impedance.  $L p$  is, therefore, equal to 2,000 ohms, and the ratio of  $L p/R$  is very large; and when you substitute the value of  $p$ , which is about 500, you get a ratio of  $L$  to  $R$  which is something like unity, or only one-twentieth of the time constant of the coil given by Dr. Fleming. But if you substitute for the value of  $R$  the value which corresponds with the energy absorbed by the transformer, and then find the corresponding value of  $L$ , the

time constant would be very much less—only about  $\frac{1}{1,000}$  of what Dr. Fleming gives. Dr.  
Sumpner.

This fact—that the resistance and self-induction in these equations must be determined in this way—was pointed out in a paper by Professor Ayrton and myself, presented to the Physical Society some time back.

Another point I wish to refer to. Dr. Fleming has made no allusion whatever to Mr. Swinburne's view of this question. Mr. Swinburne some time ago put forth the view that, assuming very little magnetic leakage in the transformers, the rise of volts which was supposed to take place on the Deptford mains must be due, not to the action of the condenser on the secondary of the transformer, but to the action of the condenser on the self-induction of the armature of the dynamo. The same so-called resonance theory really accounts for both effects; for it accounts for the rise of pressure owing to the action of the condenser on the self-induction of the secondary of the transformer, and it also accounts for the rise of volts due to the action of the condenser on the armature of the dynamo. Mr. Swinburne's view was that with such large transformers as were in use at Deptford there was very little magnetic leakage; and if that were the case, the coils of the transformer could not act as if they had self-induction—the change ratio of the transformer would be fixed—and, therefore, any effect observed must be due to the action on the dynamo. A great number of experiments were made at the Central Institution to test the relative importance of these two effects, and they are recorded in the paper read before the Physical Society. We came to the conclusion that, in our case at all events, the effects were about equal. Roughly speaking, half our rise was accounted for by leakage or self-induction in the transformer, and the other half was due to the condenser action on the armature. In the abstract of this paper which has been printed in the technical journals, we summarise our conclusions as follows:—Whether the transformation be up or down, the percentage rise of volts in the secondary is always greater than that in the primary. The percentage rises diminish as the

Dr.  
Sumpner.

secondary current increases. The percentage rises increase with the ratio of transformation. They are appreciable in the secondary when they can no longer be measured in the primary, and they still persist even when large currents are flowing. Dr. Fleming does not seem to be aware of these results, and makes no allusion to them, and unfortunately has not given us any data to show which effect is of the greatest importance in the actual Ferranti mains. His experiments show that owing to the action between the condenser and the secondary of the transformer the volts may increase by as much as 15 per cent., but there is nothing to show whether there is any effect in the armature itself. This could have been tested by taking the potential of the dynamo before the mains were switched on, or by taking it after they were switched off. There might have been some difficulty in switching off the mains when the full-load current was on. For instance, when the load in the tank was 116 amperes, there may have been some difficulty or danger in switching off the mains and taking the potential of the dynamo before and after switching off; but, at all events, the potential could have been taken before the mains were switched on, and any rise in pressure which took place could have been observed. When only the condenser current was flowing in the mains there could not have been any difficulty in switching off the mains. The engines could not have noticed the difference, because, of course, no power, or appreciable power, would be absorbed in the condenser. But although Dr. Fleming has given no particulars to enable us to compare the relative importance of the two effects, his experiments show that what Mr. Swinburne imagined was not quite correct. Mr. Swinburne said, in the discussion at the Physical Society, that with such large transformers as at Deptford there was very little magnetic leakage. On the contrary, there must be considerable magnetic leakage to account for the effects obtained.

Mr.  
Blakesley.

Mr. T. H. BLAKESLEY: I do not know that I have very much to add to what Dr. Sumpner has said upon a subject which perhaps I should have mentioned if he had not done so. The measurement of the coefficient of self-induction by any theory of

alternating currents seems to me an especially dangerous operation when we have anything like iron magnetised in the field. Mr.  
Blakesley.

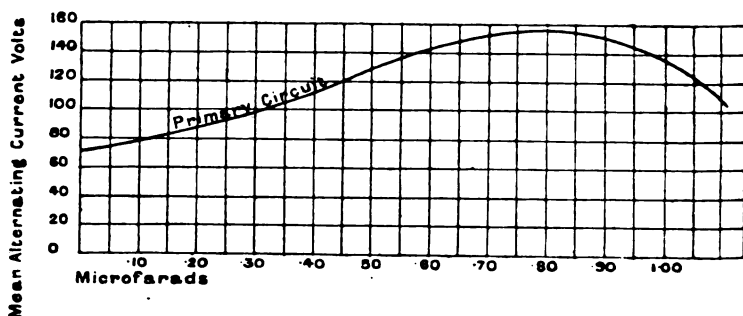
I have myself suggested methods of measuring coefficients of self and mutual induction and capacities of condensers in cases where that was not the case, but I am quite alive to the fact that when iron is involved these methods are not applicable, and I think the coefficients of induction not only cannot be measured, but can even hardly be conceived. At all events, we should have to look upon these coefficients of induction as so variable in cases of transformers in action that until we know more about the actual variations of the currents and the magnetic fields involved we can hardly adopt such a process for measuring those coefficients, or, perhaps, those condensers. I remember a striking case in point. It was in 1886, at Messrs. Nalder's works, that I tried one of those methods for measuring these coefficients of self and mutual induction in an ordinary old-fashioned transformer of the Gaulard & Gibbs type. In that case the two coils were interwound one with the other, and the two coefficients ought to have come out equal; but, as a matter of fact, the coefficient of self-induction was to the other one, the coefficient of mutual induction, in the ratio of 3 to 2, instead of being in that of equality. I should hesitate very much to apply these methods in cases I have mentioned where there is much iron in the field. The capacity effects I have also touched upon, as perhaps is known to some members here present, and I have arranged and observed cases of the increase of current-effect some years ago. Such cases have lately been drawn into comparative notice by what is called the Deptford, or Ferranti, effect. When I published my results I had a very good reason for not connecting this effect with the Deptford effect. That reason was that Deptford was not then invented. Since then, of course, it has been invented, and the thing has been brought into notice. I have not the smallest doubt that this curious effect connected with induction and capacity will play a very important part in the designs of future installations. But that effect was hardly unexpected by me. I remember mentioning it to Mr. Brougham some years ago, when the Deptford scheme was in contempla-

Mr.  
Blakesley.

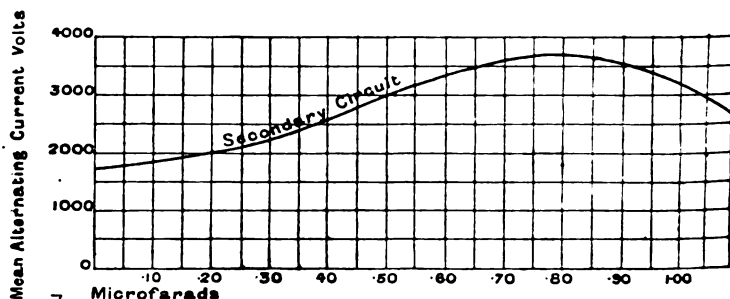
tion, and I told him I thought there would be some unlooked-for effects arising from it in precisely the way they have arisen. However, I am glad to see they have taken place.

Mr.  
Siemens.

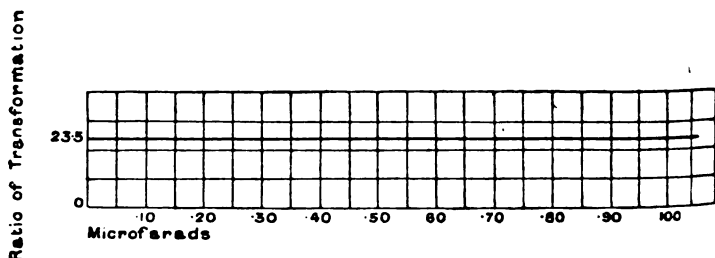
Mr. ALEXANDER SIEMENS: I would like to allude to the curves shown by Dr. Fleming, which represent some experi-



*Capacity Curve of Transformer No. 1.*—Frequency = 100  $\sim$ . Ratio of transformation = 23.5 = constant throughout. Primary E.M.F. of  $W_1$  machine without capacity = 75 volts.



*Capacity Curve of Transformer No. 1.*—Frequency = 100  $\sim$ . Ratio of transformation = 23.5 = constant throughout. Primary E.M.F. of  $W_1$  machine without capacity = 75 volts.



*Transformer No. 1.*—Curve showing Ratio of Transformation compared with Capacity in Microfarads. Frequency = 100  $\sim$ . Primary E.M.F. of  $W_1$  machine without capacity = 75 volts.

ments made at our works. I intended to bring some more curves here this evening, representing the results of further experiments, and with the permission of the meeting perhaps they may be put into the Transactions. We have represented in Fig. 6 the alteration of the ratio of transformation in that particular form of transformer; but if we alter the form of transformer, we find that the ratio remains absolutely the same, while the capacity of the secondary circuit is altered over about the same range; and I think the fact may be worth noting.

Professor AYRTON: It would interest us extremely if you would give us any idea of the difference in the two forms of transformers.

Mr. SIEMENS: The first was a transformer with a closed magnetic circuit, and the second was a "cable" transformer—i.e., having an iron core on which the primary and secondary circuits are wound, and the whole being put together on a cable machine and having an open magnetic circuit.

Mr. SWINBURNE: I would like to call the attention of the Institution to the great importance of many of the practical questions discussed by Dr. Fleming. Though I think it is unlikely that people will accidentally have enough capacity to cause damage, capacity may be made a very useful servant. I have been for some months experimenting on condensers for counteracting the action of lagging currents in increasing the armature currents unduly at central stations.

I do not think capacity effects will give rise to difficulties in coupling machines in parallel. Properly designed step-up transformers will not of themselves increase any capacity effects, but a dynamo will have to be excited to the pressure it is to give after it is in parallel. The usual practice when another machine is to be put on in a 2,000-volt installation is to excite it to 2,000 volts on open circuit, and then couple it on, and then to increase the field till it does its share of work. If the dynamos have all drooping curves, large self-induction, or great armature reaction—it matters little what it is called here—the machines already running must also have their fields weakened a little, as they now have smaller armature currents, otherwise the pressure will rise above

Mr.  
Swinburne.

2,000 volts. Suppose, however, that there is an extensive system of concentric cables with thin dielectric, or a large condenser in circuit, so that the machines actually give a higher pressure, even though working lamps, than they do when run disconnected with the same excitation. The new machine to be added must have a weak field, so as to give less than 2,000 volts before being switched in. If excited to 2,000 volts before switching in, it will take more than its fair share of load if its engine will give it; and whether the engine does or not, it will increase the pressure of the whole installation, and may damage lamps.

I think it is simplest, for rough work at any rate, never to deal with currents differing various fractions of a period in phase, but to imagine all currents split up into components either in step with the pressure, or leading or lagging a quarter of a period. Dynamos and motors may then be treated as choking coils or as condensers, according to the excitation of their fields. A motor with a weak field acts like a choking coil. Put on work, and it behaves like a choking coil with a resistance in shunt to it. Strengthen the field, and it behaves like resistance only; strengthen the field more, and it behaves like resistance with capacity in shunt to it; and so on. With no load and a strong field it behaves like a condenser. I have lately been working out a case of transmission of a large power at high pressure over a long distance. The dynamo worked a step-up transformer, and there was a large resistance loss in the leads; then came a step-down transformer, then a motor; but there were lamp circuits in shunt to the motor. A difficulty cropped up. What with the drooping curve of the dynamo, the loss by resistance in both transformers, the loss in the leads, and the drop in the transformers, the lamps would have too large pressure variation to work. I found, however, that I could get constant pressure at the motor end. By strengthening the field of the motor I could alter the ratio of transformation of both transformers and reverse the "drop," and could also control even the terminal pressure of the distant dynamo, though the motor merely received power. In fact, the motor can be made to act

as a condenser or as self-induction, with or without resistance in shunt. Or, in other words, it can be arranged so that one component of the current strengthens the dynamo field and weakens the motor fields. You always get a clearer idea of what happens by looking at these things from more than one point of view.

I think we owe Dr. Fleming a great deal for going into the whole question of capacity actions so thoroughly and completely.

Dr. FLEMING: If this discussion has shown anything, it has certainly shown how much there is to discuss in these matters which I have attempted imperfectly to bring before this Institution. Dealing, then, with some of the criticisms which have been made, and beginning with the last first, let me thank Dr. Sumpner for adding some explanations as to the limitations which must be imposed upon the symbols  $L$  and  $R$  which have been used in the formulæ given. All that he has said is perfectly correct on this point. One practical difficulty in applying these investigations to predict the results of certain combinations of inductive and permissive circuits, or circuits having self-induction and capacity, is the difficulty of predetermining or experimentally discovering the proper value of the inductance and resistance to insert into our formulæ. It is quite wrong to take the steady values of these quantities as ordinarily measured.

Passing next to Mr. Swinburne's remarks, I may say at the outset that I do not like the term "Ferranti effect" as applied to these phenomena. The effects were, as stated, noticed prominently in laying the Ferranti cables, but in nature they were well known before that date. Mr. Swinburne has presented a theory of these effects which he calls an armature-reaction theory. I am sorry to say that, as yet, the full meaning of his explanation has not become clear to me. Mr. Evershed held out the hope that further pondering would result in a clear comprehension of the value of this mode of regarding the meaning of the effect; but at present I must confess my inability to see how it comes to our aid in explaining the effects which are observed. Mr. Swinburne seems to consider that the inductance of the trans-

Dr  
Fleming.

former or of the alternator has very little to do with the increase of pressure observed when a circuit having capacity is attached to that generator. If so, how does he explain the fact that this rise of pressure disappears when a machine having very small inductance is so employed? The 1,200-horse-power alternators at Deptford, which have very small armature inductance, do not exhibit any very sensible rise of pressure effect when connected to the tubular mains; whereas the smaller alternators, when worked with step-up transformers, do, as I have shown you, exhibit it in a very marked degree. A theory, to be worth anything, must explain how it comes to pass that the armature inductance is a factor in producing the effect. It must also explain the alteration of change ratio in transformers. Mr. Siemens pointed out a moment ago that a transformer could be constructed so that it showed no change of change ratio when employed with concentric mains. We shall be desirous of hearing more about this matter.

Professor Ayrton made one or two minor criticisms at the outset in his remarks which may be accepted. In speaking of a numerical illustration which I used, taking certain assumed values of the inductance and resistance of a coil, he considered I had taken a wildly improbable case, and that no real circuit could have a time constant of 20 seconds. Mr. Preece defended my assumption, and after the discussion Mr. Bate drew my attention to the fact that he had found a very considerable time constant in the field magnets of one of the old large Edison dynamos. A number of these magnets, which consist of soft iron bars, roughly about 6 inches in diameter and 5 or 6 feet long, lie piled one above the other at the Edison-Swan factory. Taking a group of these so arranged as to form a closed magnetic circuit, it was found that an impressed electro-motive force took from 5 to 9 seconds to produce the full current in the circuit. Hence it is clear that it is quite within experimental possibilities to construct a magnet having a time constant of 20 seconds, and that there is no such improbability about my suppositions as Professor Ayrton suggests. Professor Ayrton asked me how I explained the difference between the calculated and observed

values of the condenser currents in the experiments made at the Standardising Laboratory. You will see, on looking at those experiments, that the result is to indicate that the capacity of the condenser as determined by the alternating-current method is greater than its capacity as determined by the ordinary steady-current method. This is what would take place if the condenser leaked, because any true conduction through the condenser would make the capacity as determined by the alternating currents too large, and that by the ordinary method too small. It is not certain, however, that this difference is due to conduction, and the matter needs further investigation before we can come to conclusions on the cause of the difference.

Mr. Evershed followed Professor Ayrton, and, amongst other remarks, objected to the quantity,  $pL$ , in the expression for the impedance being called a resistance. There is no doubt that it is logically more correct to limit the use of the term "resistance" to the dissipative quantity, but it is not correct to speak of  $pL$ , as Mr. Evershed seemed to do, as an electro-motive force. It is only one of the factors of electro-motive force, and only becomes the measure of an electro-motive force when multiplied by a current-strength.

The practical aspects of the matter I have brought before you have only been lightly touched upon by the various speakers. There can be but little doubt that when step-up transformers are used in connection with concentric mains, some caution should be used in arranging large systems of distribution, and as far as the observed effects go, they point to the advantages of employing high-pressure dynamos direct coupled to the mains. In conclusion, let me say that if I have not dwelt at any length on the theories or views of other previous writers on this subject, it was solely because, the space and time at my disposal being limited, I thought I could better employ them by giving you new facts and figures rather than in discussing the antecedent history of the subject.

The PRESIDENT: I have now to invite remarks on Mr. Preece's and Mr. Crompton's papers, it having been decided to take the discussion upon them together.

Dr.  
Fleming

Mr. Adden-  
brooke.

Mr. G. L. ADDENBROOKE: As Mr. Preece's paper came first, naturally the discussion should first deal with it. But the object of Mr. Preece's paper, as he says in his first paragraph, was in a large measure to point out the unquestionable economy of the high-pressure principle. Now Mr. Crompton's paper, on the other hand, directly impugns the economy of the high-pressure principle, and therefore it seems to me worth while running through his calculations in order to get at some figures which he has not given fully, and to see how they bear out his statements. If you will allow me a few minutes, therefore, I will try and take up a few points in his paper. If you turn to the first page, about half-way down, you will see Mr. Crompton says: "As it was evident that there is a certain length of feeder at which the low-pressure direct system fails to compete successfully in first cost and that of upkeep with the above-described high-pressure A.T. feeder, I have recently investigated the question in order to find out at what mean length of feeders the low-pressure and high-pressure systems compete on equal terms of first cost, cost of upkeep, and efficiency. The result of my investigation is to show that when a three-wire low-pressure system, distributing direct to 110-volt lamps, is compared with an A.T. system, distributed by 2,000-volt feeders, transformed to supply the same three-wire network, the average annual efficiency of the feeders in both cases being 90 per cent., the low-pressure feeder is cheaper for all distances up to 2,400 yards, each of the feeders being calculated to supply 100 kilowatts." The first point which struck me as being desirable to find out was the size of main Mr. Crompton had taken for his high-pressure cable, and I proceeded in this way. Mr. Crompton wishes to transfer to the other end of his cable 100 kilowatts at 2,000 volts, consequently the current will be 50 amperes, and the average efficiency 90 per cent.; and from what he says down below I take that as the efficiency over the whole of the day. However, in order to simplify the calculation, I simply take it that the minimum efficiency of the feeder shall be 90 per cent.—that is to say, 10 per cent. loss on the feeder when it is conveying a maximum current of 100 kilowatts.

Now, if we are to lose 10 per cent, on the feeder, and the voltage is 2,000, that means to say we are going to lose 200 volts in the feeder with a current of 50 amperes. If we have that, we can afford to make our resistance 4 ohms. Therefore I anticipated that the total resistance of Mr. Crompton's conductor of 2,000 yards would be 4 ohms, and I thought if I took the quotations for the highest class of Silvertown cable with double wire it would be about the same as a concentric cable. Looking out the size which would give a resistance of 4 ohms, I found it was 7/14, and, taking the gross price as given in the price list, I see it comes to £165 per mile. Mr. Crompton has given here, on the second page, the cost of high-pressure cable laid—I think we may take the discount to represent the laying—and the cable costs £165 per mile. Consequently it will be found, with the quantity of cable Mr. Crompton requires, the cost would be about £440, instead of £1,300 as Mr. Crompton puts down. Of course that alters the calculation entirely. It brings the cost of the high-tension feeder down to about half that of the low-tension. And there are one or two other points in this calculation to which also it is desirable to call attention. For instance, Mr. Crompton has put down the same cost for excavation for low pressure as for high. I think that is a great mistake. If he were going to put down a bare copper conductor, he would use a much more expensive culvert than for two 7/14 insulated cables. Therefore I think that this item might be at least reduced to £500. Transformer apparatus and cost of fixing I think also too high. I think, altogether, we may fairly assume the cost of a high-tension feeder on Mr. Crompton's basis is exactly half what he puts down for the low-tension, instead of being very nearly the same. But I would like to further point out that this is not a practical case at all. Certainly, in low-tension stations it is usual to allow a drop of 10 per cent. on the feeders, and of course with a maximum load more than that—15 or 16 per cent. But on the high-tension system it would not be for lengths of 2,400 yards usual to take a drop of more than  $2\frac{1}{2}$  per cent.—that is, just a fourth the drop Mr. Crompton anticipates. Supposing we do take a drop of  $2\frac{1}{2}$  per cent.—which would be a

Mr. Adden-  
brooke.

Mr. Adden-  
brooke.

practical case—in laying down a system it will be found, taking a cable four times the size I have mentioned, the cost just actually comes to £1,300, as Mr. Crompton has put it down. Then, however, the position is this: we have a high-tension cable costing somewhat less than the low-tension cable, and we have a loss of  $2\frac{1}{2}$  per cent. in it, as against a loss of 10 per cent. As Mr. Crompton has gone into the cost of upkeep, I have just worked that out to see how it stands. I am not taking average efficiency. I am going to state the case a little more against myself, for the sake of simplicity. I take it in this way: 100 kilowatts passing continually through the cable means 2,400 Board of Trade units a day. Mr. Crompton divides an average day into 100 parts, and says the total load hours are equal to 10 of those parts, or the load is equal to 100 kilowatts for one-tenth of the day—that is to say, a maximum current for  $2\frac{1}{2}$  hours, which is about right for an ordinary station. Taking this load factor, we see that a feeder under ordinary circumstances will supply 240 Board of Trade units per day.

Now in the low-pressure feeder Mr. Crompton will lose 10 per cent., or 24 Board of Trade units per diem; in the high-pressure case he would only lose  $2\frac{1}{2}$  per cent. Therefore 18 Board of Trade units are saved a day on the high-pressure feeder; and if we take that at 6d. a unit, we shall find a saving of £164 a year, which Mr. Crompton would simply waste in his cable. Therefore, on both accounts, the high-pressure cable is very much more economical.

Mr. Kapp.

Mr. KAPP: Mr. Addenbrooke has hardly put the case strongly enough in favour of the high-tension feeder. All the calculations which Mr. Crompton has put before us are to a certain extent vitiated by the neglect to take into account the extra cost of the plant to make up for the loss in the feeder. Mr. Crompton assumes for the feeder 90 per cent. efficiency all the year round, which means a loss of something like 30 per cent. when the station is working at its greatest output. In other words, you have to put down 30 per cent. more boilers, more dynamos, more engines—in short, 30 per cent. more of everything, including wages—for the privilege of working low pressure; and you have to

add to the figures given in Mr. Crompton's table the interest and depreciation on the extra 30 per cent. of generating plant. Mr. Kapp.

Major-General WEBBER: I should like to say a few words in support of what has emanated from Mr. Addenbrooke and Mr. Kapp, and to draw the attention of the meeting to one part of Mr. Crompton's paper where he emphasises the advantages which he believes exist in favour of his line of argument—I will not say in favour of the low-tension system. He says: "I have recently investigated the question in order to find out at what mean length of feeders the low-pressure and high-pressure systems compete on equal terms of first cost." Below that, he says that his argument "shows the absurdity of imagining that any saving can be effected by this mode [the high-tension mode] of feeding a network, and it is quite evident that those who have advocated the system have never seriously looked at the matter from this [his] point of view." This statement, I must assume, appears as rash in its character to the meeting as to me. Major-Gen.  
Webber.

Mr. Crompton also wishes us to believe that the advocates of alternating transformer systems of distribution "appear at last to be in agreement with those who designed low-pressure systems, in that they admit that for the supply of towns of ordinary compactness a low-pressure network must be provided." He refers to Mr. Ferranti by name, and overlooks the fact that Mr. Ferranti in 1885 described most accurately, at least so far as it could then be described, such a system as Mr. Crompton says has dawned upon us only lately, and the absurdity of which he says is evident following on the argument based on the figures which he has laid before us. I can only regret that Mr. Crompton has ventured to submit such wild statements to this Institution.

I have fully followed, and endorse, those points brought before you by Mr. Addenbrooke and the subsequent speaker. But one condition which Mr. Crompton puts forward in column B of his table (I am sorry to be so much down on him in his absence; I wish he were here) I cannot overlook. In this he admits that one-quarter of the length of his 2,400 yards of main must be drawn into pipes with the copper insulated. He does not, I see,

Major-Gen.  
Webber.

claim that the culvert with bare copper shall exist for more than three-fourths of the distance. Well, what does practice show us in many parts of our towns? Why, that the culvert cannot find space perhaps for more than one-fourth of the distance. If so, the remainder—namely, three-fourths—would have to be insulated with what he calls low-pressure insulation, laid presumably in tubes. In such a case this would raise his outlay of £1,020 by some £2,000, and the figure at the bottom of the column would be more like £5,000. If we admit for one moment that the total of that column B might be anything between £2,000 and £5,000, then the figure at the bottom of the next column—namely, interest at 5 per cent.—must be very much upset.

But there are figures in Table II. against which I must vigorously protest, and that is the estimate of upkeep. I do not think that they at present admit of proof. Such figures I here criticised last winter. Mr. Crompton only charges 1 per cent. for his culverts and pipes, and for insulated cable he puts a charge of 8 per cent. I am sure that no experience has yet shown that these figures can be for one moment admitted as a basis for calculation in such claims as these put forward in favour of low-pressure system of feeding as against high-pressure.

I will not detain the meeting by drawing attention to the confusion existing as to what is meant by the 10 feeders, or whether those 10 feeders emanate from the point of conversion: how they come into the calculation I cannot quite discover. I can only regret that Mr. Crompton, who is a Vice-President of this Institution, before he gave his most interesting paper in this room to the Institution of Civil Engineers, and, as it were, cast this paper (which was read without notice this day week) into the proceedings of this Institution, overlooked—which, I hope, was perhaps the case—that rather to this Institution was due that very interesting communication, which we should have all been most happy to discuss, than the hastily compiled production he has left for us to discuss to-night.

Sir WILLIAM THOMSON: One question occurs to me with reference to both Mr. Crompton's and Mr. Preece's papers.

Sir William  
Thomson.

What is the ratio of the number of units of energy supplied to and paid for by the consumer to the total produced at the generating station in each of the two systems? When practical answers to that question are taken into account, I think the result will tell considerably in favour of the low-pressure direct-current system in all cases in which it is applicable, and will lead us rather to stretch the distance by which we should prefer to choose it than give any bias in the direction of choosing the other system.

Sir William  
Thomson.

There is just one other point, and that is, the economy of copper in the conductors. Ideas seem to be ripening towards carrying out low-pressure network, to be fed either directly by low-pressure feeders on the continuous-current system, or by high-pressure feeders with transformers on the alternating high-pressure system. Now it does not seem to have been taken into account that in a low-pressure network we must have very large conductors to feed anything like a dense district, say any part of London. Whenever we go above wire of perhaps 2 or 3 centimetres—whenever we have a wire equivalent to continuous copper of circular section exceeding 2 or 3 centimetres in diameter—we have a very great loss of efficiency indeed over the copper in the alternating-current system. Take, for example, an alternating-current system at 80 periods per second. With a continuous copper wire of 2 centimetres diameter the effects of the conductance of the copper is less by 8 per cent., or the ohmic resistance—the ohmic effect of resistance of the copper—is more by 8 per cent. for the alternating current than for the continuous current. If we take 3 centimetres, the effect of ohmic resistance is 31 per cent. greater with the alternating current than with the direct current. If we take current at a frequency of 100, then 2·7 centimetres diameter of copper would entail a loss of 31 per cent. If we take such a powerful conductor as that suggested in Mr. Preece's paper, of a square inch diameter—that is, round wire of 2·9 centimetres diameter—the ohmic resistance of such a wire as that actually suggested by Mr. Preece would be nearly 30 per cent. more for the alternating

Sir William  
Thomson.

current of the lower frequency—80—than for the continuous current. These things must be taken into account for estimating low-pressure networks. If a conductor for 500 amperes, consisting of a large copper wire cable with a central strand of copper wires surrounded by six similar strands, were altered by taking jute instead of copper for the central strand, it would be found, with alternate currents of 80 or 100 frequency, that the cable with the jute core would conduct practically as well as the cable with the copper core.

I may say I feel, on the whole, that there seems to be what, to my mind, is not quite a practically judicious tendency to rush to alternating current, and to give up the simple and convenient and direct system of steady current and low pressure. I cannot but think that if, irrespectively of division among companies, the whole of London were worked out for electric light, there would not be alternating current anywhere—there would be nothing but the low-pressure direct system. I feel it is due to Edison to name his name in connection with what I believe to be the best system, which nine years ago he proposed for the supply of electric light and power to cities—the system in which consumption districts are connected to one another and fed by one or more central stations at pressures of perhaps 15 per cent. above the pressures that are to be used in the consumption circuit. I am perfectly aware of the reasons that have dictated the choice of the alternate-current system in many cases. I know also how splendidly successful it has been in some cases. Both in London and elsewhere the alternating-current system has done, and is doing, what could not be done by any other system. I do not wish it to be said that I do not appreciate the real merit of, and the great advantages obtained in some cases by, the alternating-current system. But I do think, looking to the future, we must consider that the direct-current system, with its great simplicity, and its thoroughly convenient and economical use for power as well as light, is destined to predominate, and ultimately to supersede alternating current for all densely populated districts.

There is a common idea that it will pay to put the generating

system at a distance of several miles away, in order to obtain cheaper coal and possibility of condensing water for the engines. I think, if practical men consider the subject in all its bearings, they will find that the few shillings per ton saved on the coal is far short of compensating the disadvantages of the more distant station; and what has been done hitherto in the use of condensing water, even when freely available, is, I believe, *nil*. I do not know of a single case in which triple or quadruple expansion condensing engines are used for electric lighting, even although the station may be on the side of a canal or river. I do not say it would not be a very great advantage if, by the use of an abundance of condensing water, we could get an economy equal to that of marine engines taking  $1\frac{1}{4}$  lbs. of coal per indicated horse-power per hour, instead of the 3 lbs. or 4 lbs. taken by the non-condensing engines at present in use. The varying loads during the 24 hours, and other specialities of the requirements for electric supply, seem, however, to render it impossible, at present, to realise practically in an electric light station anything approaching to the splendid economy of marine engines.

Sir William Thomson.

It is satisfactory to know that all these details of engineering are in good hands; and we may look forward to better and better results as the work extends. But I return to this: no great saving has ever yet been realised on the expense for the same power obtained by putting the generating station at a distant place. On the other hand, it is often difficult and expensive to get a site in the middle of a town, and it may be necessary on this account to put the station a little way off. No other consideration whatever ought to weigh with engineers against putting the generating station as near as possible to where the light is to be used, and doing away with every possible complication between the central station and the consumer.

Mr. A. SIEMENS: I can only fully endorse the remarks which have fallen from Sir William Thomson, that the principle which should be carried out in designing a central station is simplicity; and I think the discussion of this paper and the paper before the Civil Engineers has proceeded far too much on the special lines,

Mr. Siemens.

Mr.  
Siemens.

low-pressure *v.* high-pressure systems. An example of the low-pressure multiple stations to which Sir William Thomson alluded just now may be found at Berlin. There are, I think, five central stations, which are connected to a network fed by low-pressure currents; and that system has worked exceedingly well for very nearly seven years, during which time it has been greatly increased. In considering such an undertaking the practical points have to be looked at as well as the theoretical points, and as an example of that I would allude to what Sir William Thomson said of the great advantage of leaving out the inner core of a stranded conductor and putting jute instead of copper. Now it is perfectly true that that can be done, and that you would use a little less copper to obtain the same effect; but when the London Electric Supply Company asked us to design some of their distributing mains which were to carry a current of 2,500 volts, and to follow out Sir William Thomson's recommendation of putting jute into the inner conductor, we went into the question, and found that that would oblige us to increase the total diameter of the cable, and then the practical question arose, Which is the cheaper? Shall we put a little more copper into the inner conductor and have our dimensions smaller? or shall we do the theoretically right thing and put jute in the centre of the inner conductor? We found to consider the theoretical conditions only would be more expensive than the other, and we have not put jute in the inner conductor. Now such questions frequently arise in connection with central stations, and in deciding them we have to keep in view that these undertakings are not embodiments of theories, but are commercial undertakings, to be designed and carried out on strictly commercial lines. You should design everything upon the basis of whether it pays or not. If it pays, do it; if it does not pay, do not do it. I would also add my protest to that of General Webber against Mr. Crompton's assumption that the upkeep of low-pressure and high-pressure cables ought to be put at 8 per cent. The lead-covered cables which form the low-pressure system—at least, middle-pressure system, I suppose I ought to say—we have put down for the London Electric Supply Company, and the low-

pressure system we have put down for the Bradford Corporation, <sup>Mr. Siemens.</sup> have not cost a single farthing for upkeep; and I consider that the rate for maintaining these cables should be calculated at least not higher than the rate at which Mr. Crompton puts the upkeep of his culverts, and probably much lower.

Mr. SWINBURNE: With regard to the skin effect—or Thomson <sup>Mr. Swinburne.</sup> effect, I think it might be called, as Sir William has impressed it on us in connection with cables and transformers—it can be avoided by using, not the ordinary cable, made up of single wires, but a cable made up of smaller cables. For instance, some cable may be made up of single wires, and six of these may then be twisted over a dummy core. The mean distance from the centre is then the same for all wires.

Sir WILLIAM THOMSON: Insulated from one another?

Mr. SWINBURNE: Yes, by the thin coating of dirt and oxide that is always present. If the pitch of the twist of the wires forming each of the six cables is small, the pressure between any two will be very minute.

Sir WILLIAM THOMSON: No doubt Mr. Swinburne would be <sup>Sir William Thomson.</sup> quite right if each portion of wire got its share of being central and outside. But no cable hitherto made fulfils this condition. When the cable is made up, as it often is, with a central strand, and six other strands laid on around it, then taking away the central strand and putting jute in place of it would not make any sensible diminution in its conductance for alternate currents with the practical frequencies, if the outer diameter exceeds, say,  $2\frac{1}{2}$  or 3 centimetres.

Mr. SIEMENS: I was only alluding to the special case we were speaking of.

Sir WILLIAM THOMSON: Coming down to  $\frac{1}{4}$  inch diameter, Mr. Siemens is quite right. What I was speaking of was Mr. Preece's square inch of copper. A square inch of copper can be as well used for alternating current as for direct current; but if for alternating current, it must be spread out over a large surface. I point out that as one of the difficulties which seem to have been passed over too lightly in advocating the high-pressure system feeding low-pressure networks. I am exceedingly glad

Sir William  
Thomson.

to hear what Mr. Siemens has said about the durability of cables. It had struck me very much that we have no right to say that the expense of upkeep of cables will be greater than that for bare copper laid in culverts. It seems, indeed, that we have already some considerable practical experience in favour of the expectation that the upkeep of a properly protected cable may be practically *nil*.

Mr. Mavor.

MR. A. E. MAVOR: I would like to point out, with regard to cables of large section for high-tension alternating currents, that the method of obviating the loss of efficiency by separately insulating each strand of cable, which Sir William Thomson has suggested, may be easily and effectively carried out in those cables which are insulated by a fluid compound at a high temperature. In ordinary practice stranded conductors have all a complete film of insulation surrounding each strand, and with the addition of a single lap of cotton a high insulation resistance may easily be attained without materially increasing the section of the conductor.

In reference to Mr. Crompton's Table II., and the cost given for the supply and laying of 2,400 yards of concentric feeder, I should like to undertake the contract at half the sum he names. In the not improbable event of Mr. Crompton's conversion to the use of high-tension currents, I am sure he will go to a cheaper cable manufacturer than the one whose figures he presents to the Institution.

Mr. Preece wishes to see some of the ingenuity and energy which is being devoted to the further improvement of the dynamo and the transformer diverted into cable manufacture, and probably such augmentation of our ranks would be of advantage to the profession; but I may assure the Institution that, although it hears little of our polemics, the development of cable manufacture is at least receiving patient and persistent study from a considerable number of the members. As to what success they may attain, time, of course, must show; but it is becoming apparent that progress will not be altogether along the old lines, and that, notwithstanding the excellence of vulcanised rubber as it is now being manufactured, there are

cables employing other insulations with no sacrifice of efficiency Mr. Mavor. or permanency, and at a much lower cost.

Regarding the relative cost of cables for high- and low-tension currents, this must greatly depend on the radial thickness of the insulation which is employed; and as there is much difference of opinion as to what is really necessary, a definite specification on this point would be an advantage. The rules of the Electrical Department of the Board of Trade are a useful move in this direction.

Professor AYRTON: I do not propose to discuss either of the Professor Ayrton. papers, but merely to make a remark about what Mr. Mavor has said. Possibly I do not quite understand him, or possibly he does not quite understand the conditions necessary to be attained in constructing an efficient cable for alternating currents of high frequencies. If I understand him rightly, he is under the impression that the increase of resistance of a conductor for high frequencies can be got over if the wires composing an ordinary stranded conductor be simply insulated from one another. But that is not the case. In order to use the copper advantageously,—in order that the elementary currents in the strands may increase neither the resistance nor the self-induction of the conductor for high frequencies,—it is necessary that each wire composing the strand should have the same mean distance from the axis. That is to say, any one of the wires must be sometimes close to the axis, sometimes far from the axis; the wires being, in fact, plaited together, and not simply twisted. And if that condition be fulfilled of making every wire, on the whole, have the same mean distance from the axis, the current-density will be uniform, and the resistance and self-induction will be independent of the frequency.

Sir DAVID SALOMONS [*communicated*]: Mr. Preece, in his Sir David Salomons. paper, insists on seven conditions which he considers necessary in order to ensure safety with the electric light. One is frequent testing of the insulation. This is a condition virtually impossible in an ordinary private house, when the owner is not himself an electrician. A majority of the public would sooner dispense with the electric light altogether than have such a necessity imposed

Sir David  
Salomons

upon them. This difficulty I have had to encounter on many occasions, and in order that electric lighting should not be placed on a worse footing than gas, because of a leakage producing no smell, I have devised a simple apparatus by means of which the most uninitiated person can ascertain whether the wiring in a house is in a safe state or not. The test can be made, as often as one pleases, by the owner of the house, or even by the housemaid. It consists of a galvanometer, which may have any construction suitable for the pressure and kind of current used in the house where the instrument is placed. Below the dial there are two buttons—black and red. Three wires are brought to the instrument—viz., branches from the positive and negative mains, and an earth wire (soldered to a water pipe). On pressing the red button the negative main is put to earth through the galvanometer, and if the needle moves the leakage must exist upon the positive main. If the black button is pressed, any leakage upon the negative main will be indicated, the angle of deflection being greater as the leakage is larger. Therefore it is possible to place two red marks upon the dial to the right and left of zero, indicating the limit of deflection that is allowable in a given house, which, of course, varies with the number of lamps installed and the conditions laid down by the supply company, since such limit of deflection shows the limit of permitted leakage. If, therefore, upon pressing the two buttons successively, the needle should pass beyond either or both of the red marks, the electrical engineer should at once be called in. If these red marks are not passed, insulation may be considered safe. The contact-keys are so constructed that it is mechanically impossible to connect the two mains at the same time through the instrument. It is also possible to engrave the dial like an ohmmeter, but in an ordinary house this is not necessary. A fuse is placed in the instrument.

Mr Madgen.

Mr. W. L. MADGEN: I do not think that sufficient advantage has been taken in the course of the discussion of the opportunity which Mr. Preece has afforded of bringing out practical points in connection with the use of different forms of mains to which he concisely referred.

All the considerations tend to prove that the form of main for

electric lighting on a large scale to which all others must **Mr. Madgen,** eventually give place, will be one which can be buried in the streets in a similar manner, and with the same confidence, that gas mains are now put down.

The drawing-in arrangement, which is the leading principle in many systems of mains, is at best a makeshift, and has been rendered necessary by, or at least has kept company with, the expensive system of feeders which is a feature in continuous-current distribution.

The dispensing with channels and conduits means a considerable item, which must be taken into account in any comparison; and it has, moreover, the immense advantage of avoiding the possibility of explosions from accumulations of sewer or lighting gas, which have occurred so frequently with high- and low-tension systems in both Europe and America when conduits have been used.

Explosions of this kind have already occurred in London; in fact, when a conduit or line of pipes is opened, the presence of gas (which is found, moreover, to impair the insulation of cables laid in that manner) is very frequently detected.

Such methods are also liable, sooner or later, to dangers which arise when water is present. For instance, when bare conductors are used, and water gets access to them, they are liable to be short-circuited or injured by electrolysis. With insulated cables in pipes or conduits, the alternate presence and absence of water affects the insulation, damaging it in time; and when it is present in winter it is liable to freeze—the ice crushing or piercing the insulation, and causing partial or total short-circuitings.

Theory has already shown, and developments in modern practice are now showing, that concentric conductors are the correct form, not only for mechanical, but for electrical reasons; and as a representative of these the Ferranti main is a conspicuous example.

The long series of experiments which were tried at an early stage in connection with the paper insulation used by Mr. Ferranti, has proved that it is considerably better to build up

Mr. Madgen. an insulation by means of a large number of very thin successive layers than by single or double layers merely, as is generally done in cable manufacture. For instance, when a thick layer is used, faults, either early or late, will penetrate deeper than in an insulation consisting of a large number of thin layers, any appreciable number of which could not be defective at the same identical spot.

Many speakers spoke at length upon the fact that under alternating currents of high frequency the inner portions of a solid conductor are never reached by the current, but did not seem to sufficiently emphasise the fact that in the mains to which I have last referred, and which are in extensive use, this point has been fully met.

Mr. Preece referred to the difficulty encountered by the engineer in ascertaining the "lie," so to speak, of the demand; and with continuous-current distribution the laying down of a suitable network of mains is no doubt a considerable difficulty. It has to be done very much as a speculation, without any definite information as to the amount of current that will be required at various stages in the progress of the work, or, indeed, of how much will be finally required; and it has been to meet this that such a makeshift as a drawing-in system has found a place.

In the system of alternating-current distribution, however, developed by Mr. Ferranti, and devised by him in 1885 and subsequent years, this system is entirely obviated, in such a manner as to avoid the use of any conduit or pipe system of mains, and to make the use of concentric mains, which can be buried with confidence in the streets, not only possible, but expedient.

The arrangement may be concisely described as follows:—

The method is to lay 100-volt distributing mains on each side of the streets to be lighted, and a high-tension main on one side only. At each 200 yards, or other suitable distance, a group of transformers, placed in a brick chamber, receive the current from the high-tension main, reduce it in pressure, and feed into the low-tension distributing main on each side of the street. The

distance apart of these groups of transformers varies according to the requirements of a district. If in one particular street the lighting becomes exceedingly heavy, then all that is necessary is to put in an intermediate set, and so increase the current supply. The transformers, which always work at their best efficiency, are switched in and out of circuit as required by the automatic switch gear, and the low-tension mains are consequently fed and maintained at a constant E.M.F., the distance of the transformers apart being such that perfect regulation is maintained along the entire line to be lit. This arrangement is very easily carried out. The mains can be run through all the streets without regard to any calculated plan, and they form their own network irrespective of the positions of the houses to be lit.

The great advantage of this system is, that when a district is first lighted the mains are laid, and transformers put in at considerable distances apart, being added to as customers come on from time to time. The system can be run through any and all sorts of districts—it does not matter whether it is a very concentrated one, or one in which only a small amount of light will be at first required. It compares most favourably with the ordinary means of gas distribution, and can be extended in any direction as the demand increases.

Mr. PREECE: Mr. Swinburne was afraid that earthed concentric mains would not entirely eliminate telephonic disturbances when on full load. They certainly do so at present while the secondary circuits are in perfect order. In Rome they invariably experience disturbances when leakage comes on the secondary; but then the main is not earthed intentionally, as at Deptford. At present we have not perceived the slightest disturbance, either in London or in Newcastle. Earthing the secondary in the middle is, in my opinion, dangerous. I doubt if even the most lenient Fire Office Inspector would allow it. It materially increases the fire risk.

Sir William Thomson asked a most important and most pertinent question—"What information have we as regards the "quantity of energy made in the station as compared with the "quantity of energy sold to the consumer?" Now this is just one

Mr. Preece. of those pieces of information that we are most anxious to get, but which is the most difficult to obtain. Taking the quantity of coal burnt per unit sold, I found in one instance on an alternate-current system that while the consumption of coal per unit generated in the central station was about 8 lbs., the consumption of coal per unit delivered to the consumer was 17 lbs. That is a fact very much against the alternating-current system. But when I went to Rome last year I found that there they had kept quite accurate figures, and while they consumed from  $8\frac{1}{2}$  lbs. to 9 lbs. of coal per unit generated, they only consumed 10 lbs. per unit sold to the consumer, so that the efficiency there had been improved very much; and why? Because in Rome they have entirely eliminated waste of energy in transformers. Their transformers are banked in sub-stations, and they are turned on and turned off as the load goes on and goes off, and the result is that the great waste of the transformer system due to the arrangement now very much in use has been to a great extent eliminated.

Mr. Crompton, in Chelmsford, has told us that the pounds of coal per kilowatt-hour generated is 8.88, while the coal per kilowatt-hour delivered is 10.7. I am sorry to say that the hopeful facts given by Mr. Crompton in his paper read before the Institution of Civil Engineers are not fully corroborated by the published returns of the Company that he represented as doing so marvellously well. In his paper he showed that while on test the consumption of coal per unit generated approached something between 7 lbs. and 8 lbs., that sold amounted to something between 8 lbs. and 9 lbs., showing an efficiency in the coal bill of over 90 per cent. But I learn from the highest authority that the Company does not really get paid on more than two-thirds of the actual energy generated, and they have not yet been able to pay a dividend. The waste of energy is very great indeed in low-pressure systems.

We hope to be able to produce an indicated horse-power with a consumption of coal which shall not exceed  $2\frac{1}{2}$  lbs. No central station in England that I know of is worked with condensing engines. Popp, in Paris, does it to a great extent. I do not think it will be very long before there will be such a

station in England; and if Sir William Thomson had been in this hall at a recent discussion at the Civil Engineers, he would have heard Mr. Willans give us facts which show a great prospect of high economies in condensing engines. We shall certainly bring the consumption of coal per unit sold to something like 6 lbs. Mr. Precoe.

Sir William Thomson referred to the fact that I had not allowed for reduced conductance in the case of a square inch of copper used for alternating currents. I took one square inch purely in illustration. I did not specify the form of the conductor. The question of effective conductance in copper rods did not enter into the question. Many of you will remember that at the meeting of the British Association, the year before last I think, I showed practically by experiment that when we deal with conductors of about a centimetre in diameter there is no apparent effect of this skin resistance. Practical men have come very much to the conclusion that it is inadvisable to use mains for alternating currents, of a greater sectional area than about a quarter of a square inch. Mr. Ferranti in his Deptford mains has only a sectional area of a quarter of a square inch. In the large central station which Mr. Kapp and I are engaged in designing, there will be no single case where a feeder will exceed a quarter of an inch in sectional area; so that in practice we come well within the law Sir William Thomson has developed.

Sir William Thomson also spoke in favour of central stations scattered over a large area as against a central station concentrated at one spot; but he did not take into consideration one important fact—that when you have several central stations your working expenses are increased enormously. In the very case I alluded to just now, it became a question whether the whole of the district at Bristol should be served from one central station or from two central stations. It was evident by simple calculation that the working expenses of two central stations was much greater, *pro rata*, than of one. My own view is that one central station, if the district can be so served, is certainly better than two or three or four. And one central station at Deptford on the alternating-current

Mr. Preece. system would be far preferable to the 50 or 60 central stations which London would require if worked by low pressure. There are other important reasons in favour of extramural central stations besides diminished capital outlay—for every station must have its reserve plant—not the least important being absence of nuisance and freedom from injunction.

Mr. Siemens spoke of the great criterion of an installation being simplicity. Simplicity is the great argument that I use in favour of the alternating-current high-pressure system as against the direct-current or three-wire or the five-wire system. The law that guides us in determining this question is the law developed by Sir William Thomson. The law is clear; the calculations are definite. Dr. John Hopkinson and I and others have independently worked it out most carefully, and it is most marked and most unmistakable that on commercial grounds it is practically financially impossible to work a system with success if your three-wire system extends to a mean radius of more than half a mile. If you go to a mean radius of a mile, you must for economy use the five-wire system; if you go beyond that, then you must come back to the alternating-current high-pressure system. Mr. Crompton has not only completely deceived himself, but, I fear, many others. It is a question of £ s. d. It is not a question of alternating current against continuous current; it is not a question of Mr. Crompton against all the world: it is simply a question of what is the proper way to serve a district. You have to take that district with all its conditions, all its requirements, all its peculiarities. It may be, as in Liverpool, that for a small area the two-wire system is the best; it may be, as in some parts of London, that three wires are best; it may be that the alternating-current high pressure comes in; or it may be that you will have to use all three systems. It is a question purely of the environments of the question before you.

Mr. Crompton has, unfortunately for himself, drawn a red herring across our path. He is not consistent with himself.

Messrs. Ferranti & Sparks, writing to me after the discussion, say:—

“ From investigation of the figures in Mr. Crompton’s paper, Mr. Preece . . .  
“ we find the following with regard to his low-pressure feeders:—  
“ The cost of ten 100-kilowatt 220-volt low-tension feeders,  
“ having an average mean length of 2,400 yards and a 90 per cent.  
“ mean annual efficiency, supplying a district with a 10 per cent.  
“ load factor, should be £37,690, and the annual upkeep would  
“ be £2,930. The feeder working with a density of 750 amperes  
“ per square inch, and having a 40 per cent. drop of E.M.F. at  
“ full load; Mr. Crompton’s present figures are—Capital outlay,  
“ £28,800; and upkeep, £2,540.

“ The difference between our figures and Mr. Crompton’s is  
“ on account of our taking his own figures, as given in vol. xx.,  
“ No. 92, page 148, *Journal of Institution of Electrical Engineers*,  
“ Feb. 12th, 1891, as our basis. In this table, under the heads of  
“ ‘Crompton, 1891, Culverts,’ and ‘Crompton, 1891, W.I. Pipes,’  
“ we find that a .5 square inch cable for the three-wire system,  
“ having a central conductor of .25 square inch, costs per yard—  
“ Culvert, &c., £1 6s. 6d.; wrought-iron pipes and cables,  
“ £1 19s. 6d. Therefore a feeder of this section 2,400 yards in  
“ length, consisting of 1,800 yards of culvert and 600 yards of  
“ W.I. pipes, would cost £3,570, or, for ten feeders, £35,700.  
“ Upon the above basis, ten similar feeders .6 square inch would  
“ cost £37,690 (this section—.6 square inch—we work out from  
“ Table I., section B, of his present paper); while Mr. Crompton,  
“ departing from his figures of Feb. 12th last, gives it as  
“ £28,800.

“ These figures as to the cost of Mr. Crompton’s culvert, &c.,  
“ are more than confirmed by Major-General Webber.

“ Mr. Crompton no doubt relies on explaining the diminished  
“ cost, per yard run of culvert, by the fact that he alludes (in  
“ Table I.) to an allowance of 5s. per yard run for extra width of  
“ culverts and pipes, thus presumably proposing to lay the feeder  
“ and mains in the same culvert; but as the central station is  
“ supposed to be situated 1,800 yards away from the outskirts of  
“ the district to be lighted, and for this distance no distributing  
“ mains would be required, the benefit to be derived from laying  
“ the feeder in the same culvert with the distributing mains for

Mr. Preece. "the rest of the distance would be very small, and would quite upset his figures.

"Mr. Crompton is correct in his assumption that £290 per annum covers the cost of upkeep of his feeder, losing 40 per cent. at maximum load; but his paper is misleading, as it does not tell the whole truth. Why has not Mr. Crompton said that these feeders would require 40 per cent. more plant (in one form or another), and that the working, as well as the capital cost, would be largely increased for this reason?

"Quoting from his paper, we see he states, 'I believe the figures may be said to be sufficiently correct to make the comparison a fair one' (between the 220-volt feeder and the A.T. feeder).

"Now 40 per cent. drop is hardly a workable condition, and cannot be compared with the existing losses in actual practice. The difficulty and expense of regulation would be extreme, and the lamp breakage enormous."

His condition of a drop of potential of 40 per cent. at full load would be considered childish on the part of a less experienced engineer. In his case it is extortionate, for it forces his clients to obtain from him 40 per cent. more plant. Messrs. Ferranti and Sparks continue:—

"Mr. Crompton then compares the advantages of a 220-volt 100-kilowatt 600-yard feeder, having 90 per cent. annual efficiency, and supplying a district with a 10 per cent. load factor, with the longer feeder, and the figures are supposed to be based on Mr. Crompton's actual experience in London on similar feeders. The price is put down at £300 per feeder, and the upkeep £25. These figures, again, do not agree with those given in February last, as, working on the latter basis, the capital outlay per feeder would be £516.

"We take Mr. Crompton's figures of February, as these compare with those he gave in 1888. We do not think that these feeders could be laid in the same culvert with the distributing mains, as the difficulty of laying the ordinary culvert is great; and if an extra width were used, the proportion of pipes would be increased, bringing the net results to much

"about the figures we have taken. The maximum loss is 40 Mr. Preece.  
"per cent., but in any case the feeder would be unworkable,  
"as the density allowed is 3,000 amperes per square inch at  
"full load. The same remarks apply to regulation with this  
"feeder as in the previous one; the drop is in both cases 40  
"per cent.

"As the above is not a practical feeder, and in order to  
"compare a low-pressure with an A.T. feeder at the point  
"where the advantages are more equal, we take a feeder 880  
"yards in length, but not dropping more than 10 per cent. at  
"full load, thus increasing the mean annual efficiency of the  
"low-pressure feeder to nearly 97 per cent. The section of this  
"feeder would be nearly  $\cdot 9$  square inch, and, according to R. E.  
"Crompton's figures of February last, would cost £2,006, the  
"annual upkeep being £164 3s.; the annual upkeep amounting  
"to  $\cdot 45d.$  of a unit sold, when supplying a district with 10 per  
"cent. load factor.

"Now to turn to the A.T. feeders (2,400-volt) having a  
"mean length of 2,400 yards, and supplying a district with a  
"10 per cent. load factor, and having a mean annual efficiency  
"of about 86 per cent.: the cost of ten 100-kilowatt feeders  
"would be, including converters, £15,125, and the annual  
"upkeep £1,626; the maximum fall at full load would be  
"3 per cent.

"These figures compare with Mr. Crompton's ten 100-  
"kilowatt 2,000-volt A.T. feeders costing a sum of £24,000, and  
"having an annual upkeep of £2,855.

"The annual efficiency of the A.T. feeder can be brought  
"up to 94 per cent. by means of using an automatic switch to  
"regulate the number of transformers in circuit. This would  
"increase the cost to £16,470 10s., and the annual upkeep to  
"£1,760. The maximum fall at full load would still remain  
"3 per cent.

"For carrying out the distribution of the mains on the 2,400-  
"volt system, three feeders would be laid together from the  
"generating station for a distance of about 2,400 yards; they  
"would then branch in different directions, one feeder feeding  
"four, and the others three sub-stations, each of 100 kilowatts.

Mr Preece.

"The cost of a practical A.T. feeder having 94 per cent. annual efficiency, 880 yards in length, working at 2,400 volts, to compare with before-mentioned L.P. feeder of same length, would be £1,145, the annual upkeep being £109; the annual upkeep amounting to .3d. of a unit sold, when supplying a district with 10 per cent. load factor. The above feeder would have a maximum drop of 2·5 per cent.

"The results show that a 2,400-yard low-tension 220-volt 100-kilowatt feeder, having a 90 per cent. annual efficiency, cannot be put down for less than £1 11s. 3d. per yard, and cannot be maintained for less than 2s. 5d. per yard, with a loss of 40 per cent.; while the A.T. feeder (100-kilowatt), having 94 per cent. annual efficiency, costs only 13s. 8d. per yard, and can be maintained for 1s. 5½d. per yard."

These figures are summarised in the following table, and they give a result very different to those shown by Mr. Crompton in his strange conclusions:—

COMPARATIVE COST OF TEN 100-KILOWATT 2,400-YARD FEEDERS,  
10 PER CENT. LOAD FACTOR, 90 PER CENT. MEAN ANNUAL  
EFFICIENCY.

220 VOLTS DIRECT AND 2,400 VOLTS A.T.

SCHEME.	Capital Outlay.			Cost per Yard.			Cost per Yard Upkeep.	Cost per Unit Upkeep.	Density, Square Inches.	Annual Efficiency.	Loss at Full Load.	Section Square Inch per Feeder.	Upkeep per Annum.			
	£	s	d.	£	s	d.	s.	d.			%		£	s	d.	
Crompton ) Conduit (Revised) )	37,690	0	0	1	11	3	2	5	·81	750	88·5	40	·6	2,930	0	0
A.T. 2,400 ) Volts ... )	16,475	0	0	0	13	8	1	5½	·48	500	94·0	3	·1	1,761	0	0
COST OF 10 FEEDERS AS ABOVE, 880 YARDS LONG.																
Low Pres- } sure ... )	2,006	0	0	2	6	0	3	8½	·45	500	97·0	10	·9	164	3	0
A.T. ... ..	1,145	0	0	1	6	0	2	6	·3	830	94·0	2½	·05	109	0	0

Mr. CROMPTON, in reply [*communicated*]: I offered my paper <sup>Mr. Crompton</sup> to the Institution, although at a late period in the session, because of the extreme importance of the subject with which it deals. I purposely made it short, and endeavoured to reduce the problem I presented to the Institution to the simplest possible shape. It is perhaps for this reason that my object appears to have been misunderstood by most of the gentlemen who have discussed my paper, and who, from the tone of their remarks, do not appear either to have studied it or to have appreciated its object. With the exception of the remarks made by Sir William Thomson and Mr. Addenbrooke, the discussion, so far as it took place at the meeting, hardly touches on the important points which I laid before you; but Mr. Preece, in his reply, has included a long written communication from Messrs. Ferranti and Sparks, which I welcome as a fair criticism, and with which I propose to deal at length. I will commence by restating the position I took up in my paper. I endeavoured to show that if it be granted that a low-tension network on the three-wire system with feed points is the cheapest and best way of distributing electricity from house to house, whether the current be direct or alternating; that it is then more economical to place the generating station in the centre of each district, so as to supply it by short low-pressure feeders, than to place the station at a point external to the district, so that it has to be supplied by long feeders; and that it becomes more economical to supply by high-pressure A.T., than by low-pressure direct, when the average length of the feeders exceeds 2,400 yards: but that in both cases the extra cost of upkeep of the long feeders far outweighs the small economies resulting from placing the station at a distance. I most respectfully submit that none of the gentlemen who discussed my paper have addressed a single argument to disprove these views. Mr. Addenbrooke misses his point, because he fails to include (when he is calculating the high-pressure feeder) the loss due to the transformer at the end of it. If he had done so, he would, I think, have been in agreement with me as to the cost of the complete high-pressure feeder requisite to give the 90 per cent. average annual efficiency I

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named. My calculations allow for a  $2\frac{1}{2}$  per cent. loss in the high-pressure conductor at the time of maximum load, and, of course, for a much smaller percentage at times of average load; but the balance of the 10 per cent. average loss is calculated from the efficiency of the transformer, which Mr. Mordey kindly worked out for me. Moreover, although it has been the fashion recently to quote the case of Rome as an extremely favourable example of the efficiency of distribution attainable by the A.T. system when the transformers are banked, Dr. Fleming informs us that the actual efficiency is only 90 per cent., and that in order to obtain this the transformers are absolutely switched off during daylight hours, which would not be the case in the high-pressure feeder now under consideration.

I have been under the disadvantage of only receiving Mr. Preece's remarks containing Messrs. Ferranti and Sparks's written communication at the last minute of writing my reply, consequently I have not had time to communicate with Messrs. Ferranti. Had there been time to do this, I could have pointed out to them several errors into which they have fallen when criticising my figures, and which I am sure, with their customary fairness, they would have set right, and thus have saved me the necessity of pointing out these mistakes here. Messrs. Ferranti wish to show that the cost of my 2,400-yards feeders should be £3,769 each instead of £2,800 (the figure given by me), and that the upkeep should be increased in proportion. It is easy to point out their mistake. They have supposed that I must provide for each feeder a culvert complete, as constructed for a network, with all the special gear required for the network; whereas the figure that I have allowed—viz., 5s.—as the share of the network culvert required by each pair of feeders is an ample one for the purpose. Messrs. Ferranti do not attempt to dispute this with regard to that portion of the feeder which lies within the district to be supplied, although they do not make any allowance for it in their figures; but as regards the 1,800 yards exterior to the district, and which connects it with the generating station, there would be no such network, and they consider that I should have to build a special culvert for each feeder. It is quite certain that I

do not contemplate the necessity for this, for the following reasons. If it were the case, as Messrs. Ferranti suppose, that there was no network laid in the 1,800 yards connecting the district with the generating station, it is probable that the district traversed would be open country, or land of such nature that culverts could be employed for the entire distance, instead of part culverts and part cables, as estimated by me; again, that the culverts themselves would be constructed at a cheaper rate than they would be in the footways of the inhabited part of the town; and, again, that culverts to contain feeders only are much cheaper to construct per yard run than the culverts for the network, on account of the smaller number of surface boxes which must be provided to give access for attachments to the consumers' houses. For these reasons, if I had to carry 10 pairs of feeders through an open country or along the side of a railway embankment, I can construct it for 30s. a yard run, and include a profit to the contractor. This only gives 3s. as the cost for each pair of feeders, instead of the 5s. I allowed in my paper. The next serious error that Messrs. Ferranti have fallen into is that they have miscalculated the area of the copper I used for the long feeder, and consequently make their calculations as to fall of potential at time of maximum load incorrect. If you turn to Table I., section B, of my paper, from which they quote, you will find that the cost of bare copper is put down at 14 tons at £90 a ton, equals £1,260. The cable, which is put down at £1,020, contains 5·2 tons of copper, therefore the total weight of copper in each feeder is 19·2 tons, or, roughly, 43,000 lbs., which, divided by the distance, 4,800 yards, gives 8·91 lbs. per yard. The weight of 1 square inch of copper per yard is 11·52 lbs., so that copper of this weight has approximately ·78 square inch section instead of ·6, the figure given by Messrs. Ferranti; and of course the losses calculated by them are reduced in the same proportion. Messrs. Ferranti state, with considerable truth, that for the short periods of maximum load the great loss in the feeders will entail proportionate increase in the size of the generating plant. This is only partially true. No fact that has come before my notice during the last few years has been so noteworthy as the very

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short periods during which the load touches its maximum. I have observed that these periods only occur for a very limited number of hours in the year, the daily maximum in many cases being only reached for a few minutes. The percentage of accumulators which I have always added to complete the three-wire system has been invariably found sufficient to provide the extra output required during these short periods of maximum load; but Messrs. Ferranti must remember that the growth of the demand for electric lighting is a very slow process, and that the continuously insulated feeder is at a great disadvantage when compared with the low-pressure bare feeder in respect of having its cross section easily increased as the load on the station increases. For this reason Messrs. Ferranti are forced to put down their feeder plant years in advance of their demand, whereas it is so easy to increase the section of the low-pressure feeder that there is no necessity for putting in more copper than that sufficient to enable the maximum loss in E.M.F. to be kept down within reasonable limits. I have not taken advantage of this in comparing the merits of the two systems, but no one will deny that it is a great advantage for the low-pressure system that the cost of the copper in the feeders can be proportioned to the demand, and increased at small expense as the demand increases. Messrs. Ferranti have used the expressions "workable" and "practical" feeder. Now I say that, for the above cause, the practical feeder Messrs. Ferranti propose must be for many years too large for its work; and consequently, for this cause only, their calculations must be revised. Turning to Messrs. Ferranti's own estimate for feeders, it gives no particulars of the class of conductor they use, or whether the cost is an estimate or based on actual experience. I need hardly remind them that my estimates, at all events for low-pressure feeders, are based on actual experience, and I shall be very much interested to learn what practical experience Messrs. Ferranti have had in the use of working on banked transformers worked by automatic devices. I am under the impression that these are still in the nature of ingenious devices, which are on their trial, and therefore should not be taken into account in an argument such as this one, which deals

with the state of the art as it exists at the present moment. Taking Messrs. Ferranti's own figure, I must point out to them that the figures they themselves give prove my case. They admit that the upkeep of the 10 feeders would be £1,760 a year in place of the £240 which I gave as an outside figure for the 10 short feeders. The difference—viz., £1,520—divided by the output in units, 876,000, is equal to 42d. per unit, which, although it is lower than 72d. (the figure I gave), still remains a figure sufficiently formidable for the purposes of my argument. I therefore leave Messrs. Ferranti at this point—viz., that they have now to show how they expect to effect economies by working at the distant station which will more than compensate for this increased cost of distributing the energy. The criticisms levelled by Messrs. Addenbrooke, Kapp, General Webber, Mavor, and others at the percentages given in my maintenance table do not seriously affect the question. If these gentlemen had themselves altered the percentages to those they think desirable—say, reducing the upkeep of cable from 8 per cent. to 4 per cent., that on transformer pit to 1 per cent., that on transformers themselves to 5 per cent., all of which percentages I believe to be too low and unsafe to go on—they would have found that the upkeep of the 600-yards feeders would work out at £20 15s. 11d., that of the 2,400-yards low-pressure feeder at £208 7s., and that of the high-pressure one at £205 15s.; the balance in favour of the short feeders would still remain £1,850 per annum, or about  $\frac{1}{2}$ d. on each unit sold.

I now turn to another matter. Mr. Preece, I much regret to say, has introduced into a scientific discussion of this nature information he received at a private conversation with a gentleman connected with the Kensington Company. Mr. Preece's statement is of such a serious nature that I have communicated with him on the subject, and he now informs me that a director told him that he was quite sure the company did not get paid for two-thirds of the electricity they sent out. This enables me to identify the meaning of the conversation, which referred solely to the losses which occurred at one period of the year, due to certain meters failing to register. Mr. Preece, however, has used the

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conversation to throw doubt on the correctness of my figures used at the Institution of Civil Engineers, and it is therefore necessary to deal with them seriously. The figures given by me in my paper at the Institution of Civil Engineers were copied directly from the books kept at the Kensington station of the Kensington and Knightsbridge Company. I attach herewith a short table showing the results of the first five months of this year. The first line gives the units registered in the customers' meters; the second gives the units which are registered on the circuits but not paid for, being either used in the company's own works, it includes also the missing units, from any of the consumers' meters failing to register; the third line gives the total units registered by the station meters as sent out on the circuits after passing the accumulators; the fourth gives the total units generated at the dynamo terminals; the fifth line gives the efficiency of distribution; the sixth the lbs. of coal per unit generated; and the last line the lbs. of coal per unit sent out on circuit. It will be seen that the average distribution efficiency of the year is 81 per cent.

I have strong grounds for believing that the figures given by Mr. Preece showing the consumption of fuel at Rome must be corrected, as I am informed on good authority that the actual consumption of fuel is 20 lbs. of coke per unit, not 10 lbs. as quoted by Mr. Preece. The recently published balance-sheet of the Brompton station of the House-to-House Company shows very similar figures; and in the face of such figures I cannot understand on what Mr. Preece and Messrs. Ferranti, and others supporting the alternating transformer system, base their assumption of its superiority over low-pressure systems, even when the area to be supplied is very large.

I notice that General Webber repeats a remark that he has made before—that the culvert system cannot be applied to more than one-quarter of the length of the streets to be provided. This may be the case in Chelsea, but it is certainly not the case in either Kensington or Notting Hill. In the latter case the proportions are as I have given them in my letter—three-quarters culvert to one-quarter cable; and in Southampton it is entirely culvert, and in Northampton about four-fifths of culvert. I find

also that in other towns we can entirely dispense with the use of <sup>Mr.</sup> cable. <sub>Crompton.</sub>

KENSINGTON AND KNIGHTSBRIDGE ELECTRIC LIGHTING COMPANY,  
LIMITED—KENSINGTON COURT STATION.

	January.	February.	March.	April.	May.	TOTAL.
Units sold to customers	36,894	34,283	23,795	20,039	15,536	130,547
Units used by works, not registered by private meters ... }	1,362	918	1,588	1,454	1,486	6,788
Total units registered as delivered on cir- cuits ... .. }	38,256	35,201	25,383	21,493	17,022	137,335
Total units generated ...	48,267	42,565	30,739	27,123	20,449	169,145
Distribution efficiency...	79 %	82 %	82 %	80 %	83·5 %	81 %
Lbs. of coal per unit generated ... .. }	7·6	7·37	7·3	7·5	7·58	...
Lbs. of coal per unit registered on circuit }	9·65	8·9	8·8	9·4	9·0	...

On the motion of the PRESIDENT, a vote of thanks was unanimously accorded to Mr. Crompton for his communication.

A ballot for new members took place, at which the following candidates were elected:—

*Foreign Members :*

T. Hasegawa. | S. Hayashi.

*Associates :*

W. Fox Bourne. | John Denham.  
Charles Dalziel Copland. | Matthew Pepper.

*Students :*

Frederick William Cheverton. | James Hedges.  
George John A. Fuller. | George Hoser.  
Leonard Fuller. | Alan Patrick McDouall.  
P. R. Friedlaender. | George Owthwaite Sedgwick.

The meeting then adjourned.

## A B S T R A C T S.

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### C. DECHARME—SUPERPOSED LONGITUDINAL AND TRANSVERSE MAGNETISATION.

(*Comptes Rendus*, Vol. 112, No. 10, p. 523.)

The author experimented with a piece of steel 10 cm. by 2·8 cm., by ·3 cm. thick. This was first magnetised longitudinally, and the diagram with iron filings obtained in the usual way. It was then magnetised transversely until there was no indication of the longitudinal magnetism left in the diagram. (If this is done slowly and carefully there is an intermediate stage, in which both magnetisations show clearly.) It was then magnetised a number of times, alternately in each direction; and it was found that the more often it was repeated, the easier it became to overcome in the diagram the effect of the last magnetisation; until, when a large number of magnetisations had been superposed, the steel approached saturation, and reached a sort of unstable condition, when an extremely slight magnetising force in either direction was sufficient to obliterate in the diagram the effect of the previous magnetisation. The author suggests repeating the above in combination with what he has called circular, and with what might be called helicoidal, magnetisations.

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### C. FROMME—MAGNETIC INVESTIGATIONS.

(*Wiedemann's Annalen*, Vol. 43, No. 5, p. 181, and No. 6, p. 256.)

These are investigations on the effect of small magnetising forces on iron and steel possessing a large previous permanent magnetism. The metal under test was placed in a magnetising spiral of 1,859 turns, and not touched during the observations. Variable liquid resistances being employed, so that the current could be slowly decreased to a very small value before being broken, and *vice versa*, and a variable wire rheostat was in circuit which gave sufficient working range of currents; the wire resistance being set so as to give the current required, and the liquid ones only used to bring up the current gently and then cut out. A bundle of some 100 iron wires was principally experimented on. These were 15 cm. long and ·1 mm. diam., and were embedded with paraffin wax in a glass tube. It was then repeatedly magnetised with a current,  $C_1$ , of 1 ampere, until its permanent magnetism, P.M., had attained a constant value. It was then once magnetised with a current,  $C_2$ , about ·95 A, and the P.M. was found to be slightly increased. The previous saturating process was then repeated, followed once by still further slightly diminished  $C_2$ , and so on by small steps till  $C_2$  was reduced to about ·005 A. It was found that the increase of P.M. continued to increase as  $C_2$

diminished, till it reached a maximum of 1.1 per cent. for  $C_2$ , = about .3 A; the increase then diminished till  $C_2$  = .11 A, which had no effect either way: this the author calls the "neutral" current. After that, the P.M. diminished as  $C_2$  lessened, till there was a maximum decrease of .5 per cent. for  $C_2$ , = .05 A, after which the decrease diminished continuously with  $C_2$ .

The same repeated with a wrought-iron bar gave similar results: maximum increase, .8 per cent.; maximum decrease, .9 per cent.—both with slightly higher values of  $C_2$ . With a steel bar the maxima were .5 per cent. and .7 per cent. These two, especially the steel, were not so thoroughly saturated by the maximum current as was the bundle of wires.

Some series of figures are also given showing the effect of varying the initial maximum magnetising current. As this is diminished, the strengthening effect of the smaller currents also diminishes, even if the P.M. due to the initial current remains the same.

The effect was then tried of repeating the reduced current,  $C_2$ , several times, and the subjoined table gives the results. The top line, R, is the resistance in circuit, in addition to that of the coil (about 7.3);  $\delta_1$ ,  $\delta_2$ , being the successive increments of P.M. due to the first and second applications of  $C_2$ , and  $\delta_{3+4}$  that due to the third and fourth combined; the original value of P.M. being 802.

R.	5.	10.	15.	20.	25.	30.	40.	50.	60.	80.	100.	130.	160.	550.
$\delta_1$ P.M. ...	+ 8.5	+11	+12.2	+12.3	+12.1	+10.9	+6.2	+1.5	-1.5	- 3.5	- 4.5	- 5.3	- 4.8	-2.1
$\delta_2$ P.M. ...	+ 1.3	+ 2.1	+ 2.1	+ 1.5	+ 1.3	+ 0.7	-1.3	-2.9	-3.7	- 3.8	- 3.7	- 3.5	- 2.5	-0.8
$\delta_{3+4}$ P.M.	+ 0.8	+ 0.7	+ 0.5	0	- 0.3	- 1.1	-2.8	-4	-4.1	- 4.1	- 4.0	- 3.3	- 3	-1
Sum ...	+10.6	+13.8	+14.8	+13.8	+13.1	+11.3	+2.1	-5.4	-9.3	-11.4	-12.2	-12.1	-10.3	-3.9

Further experiments show that a repetition of  $C_2$ , even up to the twelfth time, still has an effect. When the current is reduced from  $C_1$  to a moderately small value, then raised to a medium one, and finally reduced, *via* the liquid resistances, to practically zero, and broken, the P.M. is greater than if it is reduced in one operation.

Observations were also made on the temporary magnetism (T.M.) caused by the various values of  $C_2$ , and it was found that the second and subsequent applications of  $C_2$ , whether they increased or decreased the P.M., always produced less T.M. than the first. This diminution, when  $C_2$  was comparatively large, was much greater than  $\delta$  P.M.; was comparable to  $\delta$  P.M. where  $C_2$  was "neutral" current, and became less and less compared to  $\delta$  P.M. as  $C_2$  decreased further. If  $C_2$  be applied, and then followed by a still smaller current, the effect of this latter is less than if it had directly succeeded the saturating current,  $C_1$ . This holds good even if  $C_2$  is the neutral current, and has had no apparent effect on the P.M. The diminution of the P.M. due to  $C_1$  with lapse of time is unaffected by any value of  $C_2$  having been applied.

Reverse currents were then tried, the largest being  $C_2$  = .045 A. With this,  $\delta$  P.M. was - 318 (in 800); but if previous to this the neutral value of

$C_2$  had been applied, the reduction was only 307. This effect of the previous application of the neutral current became more and more marked as the (reversed) current was diminished, till for  $C_2 = -0.004$ , the reduction after the neutral current was only 18, as against 36 when  $C_2 = 0.004$  was applied direct after the saturating current,  $C_1$ . Numerous tables of results and a few curves are given in the original paper, which is of considerable length.

**W. WIEN**—THE TELEPHONE AS AN OPTICAL INSTRUMENT  
FOR MEASURING CURRENT.

(*Wiedemann's Annalen*, Vol. 42, p. 593.)

The author employs a telephone whose diaphragm consists of a thin corrugated German silver disc similar to those used in aneroid barometers, and having a small piece of iron at the centre. A flat steel spring is fixed, by one end, above and parallel to the diaphragm, to the centre of which it is connected by a short rod. At the end of the spring is a small galvanometer mirror, which thus follows all the movements of the diaphragm. The mirror throws on the scale the image of a slit, which is broadened into a band of light when the instrument is traversed by an alternating current. The spring is carefully tuned, by altering its length, to give the same note as the diaphragm. In the make-and-break arrangement a stretched wire is used, kept in vibration by an electro-magnet. The author prefers this to using a spring, as is usually done, as it is so easy to tune it exactly to the note of the telephone. The instrument will give a deflection—or, rather, a band of light—one hundred times greater for a sine curve current whose period is in tune with it, than for an equal current not in tune. As a test, ten bridge measurements were made, on the same resistance, using first the optical telephone, and then a dynamometer which gave one scale division with  $10^{-6}$  amperes. The mean error with the former was 0.12 per cent.; with the latter, 1.1 per cent.; the error when an ordinary telephone was used was not less than 1 per cent. The author sums up his claims for the instrument—first, that it measures, with greater sensitiveness than any other, a pure sine curve current of one definite period, and ignores all other currents which may be flowing at the same time; second, the deflection (breadth of band) is directly proportional to the amplitude of this current.

**DRUDE and NERNST**—INFLUENCE OF TEMPERATURE ON  
BISMUTH IN THE MAGNETIC FIELD.

(*Wiedemann's Annalen*, Vol. 42, p. 568.)

The magnetic field employed was about 7,000 C.G.S., and the magnets were specially arranged so that the metal under test could be kept at fixed temperatures while between the poles. The bismuth used melted at  $267^\circ$ , and was very pure. The "Hall effect" was first experimented on, and it was found that it slowly decreased as the temperature rose, till near the melting point it was from one-quarter to one-half what it was at  $16^\circ$ ; at the melting point it suddenly

dropped to not more than one-sixtieth of the former value; and it is possible that even what was observed was a spurious effect, as in mercury (*infra*). It was shown by Righi in 1884 that there is an increase of resistance in bismuth when in a strong transverse field. This increase was also found to vary with temperature, the effect diminishing with increase of temperature; the increase of resistance when the magnet was excited being, roughly, 22, 8, 1, and .4 per cent. at 16°, 100°, 223°, and 290° respectively. At the latter temperature, the melting point, Weber's observation was confirmed, that the resistance of the metal suddenly falls to about one-half.

Experiments with antimony showed a similar diminution of the Hall effect, but to a much less degree. With mercury there is an appearance of a Hall effect, at ordinary temperatures, about one-three-hundredth that in bismuth. It also increases in resistance when in a magnetic field of 8,000 C.G.S. to the extent of about .2 per cent., but this may be more or less spurious as the increase of resistance varies with the current flowing through the mercury.

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**E. BRYLINSKI**—NOTE ON A SUBTERRANEAN LINE IN A CEMENT CULVERT.

(*Annales Télégraphiques*, Vol. 18, p. 163; cf. *Ann. Tel.*, Vol. 16, p. 424, and Vol. 17, p. 5.)

The line referred to consists of three cables, and runs between Lyon and La Palisse. Two sections are in iron tubes, and have given no trouble; in the third section, which is only 41 kilometres long, 394 faults have occurred since 1885, when the line was put down. The faults may be divided into two categories. The first class are due to lightning, and are easily recognised. The discharge bursts through the gutta-percha, sometimes leaving a scarcely visible puncture, sometimes destroying it all round the wire. Near the hole the conductor is oxidised and blackened, and if the discharge is violent, several of the strands may be broken. When the gutta-percha is in good condition, the discharge produces several of these faults; but they are easily localised, and when repaired the insulation of the line is as good as before. A slight discharge will sometimes lower the insulation without giving these definite faults, which can be cut out. The second class of faults is much more serious. The culverts are always more or less full of water, and sometimes, for whole kilometres together, this becomes black and putrid-smelling; the whole of the outer covering of the cables disappear, and the gutta-percha core is left bare. It appears that cement held in suspension by the water is deposited on the gutta-percha, which it partly eats away, and renders the rest spongy. A number of faults are thus set up which are difficult to localise and remove, as repairing one fault frequently starts another close to it.

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**HUTIN and LEBLANC**—ALTERNATE-CURRENT MOTOR.

(*Comptes Rendus*, Vol. 112, p. 933.)

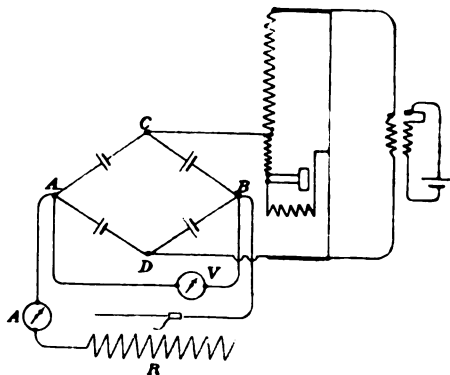
This is an alternate-current motor without commutator, working off any ordinary supply, and which does not require to be synchronised with the dynamo

It consists of two rings, one fixed and one movable, with two circuits on each; each circuit being wound to produce a like number of alternate north and south poles. To start the machine, the two movable circuits may each be independently closed through variable inductionless resistances. The two fixed circuits, in one of which is a condenser, are then joined, in parallel, to the supply mains, when the machine at once starts. As the speed gets up, the resistances are diminished, and finally cut out altogether. It then runs perfectly as a motor, provided it is not required to give a greater torque than two-thirds of its calculated maximum. A motor has been built on this principle which at  $75 \sim$  per second (the highest which could be obtained to try it with) gave 11 H.P., with an efficiency of 78 per cent. At  $120 \sim$ , for which the motor was designed, it is calculated that it would give 20 H.P. at 88 per cent. efficiency.

### J. UPPENBORN—MEASUREMENT OF INTERNAL RESISTANCE.

(*Elektrotechnische Zeitschrift*, Vol. 12, No. 12, p. 157.)

The author maintains that the internal resistance of a cell varies materially with the current flowing, and that therefore most of the methods for measuring it



give unreliable results. By his method, shown in figure, it will be seen that the resistance is measured, by alternating currents and the telephone, of four cells while they are discharging through the circuit A R. The four cells must be alike in E.M.F. and internal resistance. It will then be seen that, the points C and D being at the same potential, there is no current round the bridge due to the cells; nor, it will be observed, will the presence or absence of the voltmeter, V, or the ammeter circuit, A R, make any difference to the resistance as measured by the bridge.

### ABONS and RUBENS—THE VELOCITY OF ELECTRICAL WAVE-PROPAGATION IN LIQUIDS.

(*Wiedemann's Annalen*, Vol. 42, No. 4, p. 581.)

This is a direct investigation of Maxwell's well-known mathematical deduction that the specific inductive capacity of a substance is equal to the square root of the

ratio of the velocity of electrical waves in air to their velocity in the substance, or, as it is more frequently expressed, S.I.C. = (index of refraction)<sup>2</sup>. By a special arrangement of "Hertz" apparatus the authors have directly measured the velocity of waves 6 metres long in four liquids, and hence the indices of refraction for waves of that length. The following table gives their results:—

	S.I.C. according to			$\sqrt{\text{S.I.C.}}$	Index of Refraction.	
	Hopkinson.	Cohn and Arons.	Arons and Rubens.		$\lambda = 6 \times 10^{-7} \text{ m.}$	$\lambda = 6 \text{ m.}$
Castor Oil .	4.78	4.82	4.67	2.16	1.48	2.05
Olive Oil ...	3.16	...	3.08	1.75	1.47	1.71
Xylol... ..	...	2.36	2.35	1.53	1.49	1.50
Petroleum...	2.07	...	2.06	1.44	1.45	1.40

$\lambda = 6 \times 10^{-7} \text{ m.}$  is orange light.

### J. MOOSER—INVESTIGATIONS ON THE METALLIC FILM DEPOSITS IN VACUUM TUBES.

(*Wiedemann's Annalen*, Vol. 42, No. 4, p. 639.)

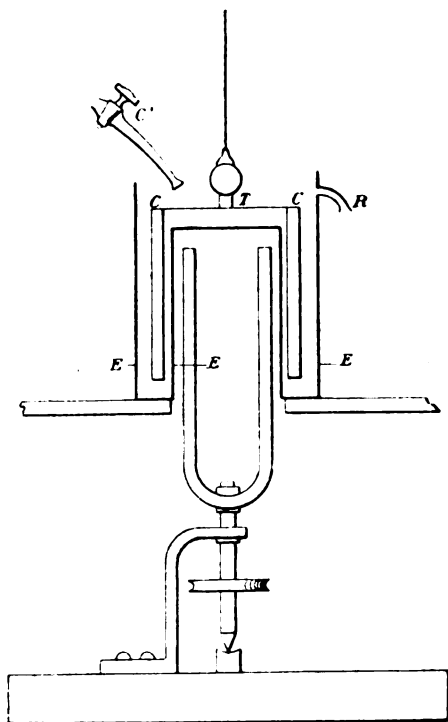
The cathode was a sphere 3 mm. diameter, generally of platinum, and the deposits were received on a horizontal glass plate placed directly beneath it at various distances. Special precautions were taken to remove oxygen as far as possible before commencing. Measurements were made on the resistances of zones of the deposit, the centre being the point plumb under the cathode; these agreed very closely in their ratios to each other with calculations based on the assumption that the discharge from the cathode was strictly radial. For measurements of the specific resistance of the deposit, it was received on the inner surface of a portion of a glass sphere, concentric with the cathode; on such a surface the deposit should be perfectly regular. The platinum cathode deposited on this about .00024 gramme in half an hour. The specific resistance of the deposited metal was 11.3 times as large as that of normal platinum; a deposit obtained in the presence of comparatively much oxygen having a resistance 82 times as great as the normal.

### Dr. d'ARSONVAL—ELECTRICAL DETERMINATION OF JOULE'S EQUIVALENT.

(*Bull. Soc. Int. des Electriciens*, Vol. 8, No. 71, p. 90.)

A thick ring of copper, *c c*, is suspended axially by a wire, as in figure, and carries a mirror, *m*. The ring is immersed in water in the vessel *e e*, and the magnet *N S* rotated at uniform speed. Foucault currents are thereby set up in

the copper, the ring rotates through a certain angle against the torsion of the wire, and the whole of the work done on it appears as heat. The liquid is kept at the temperature of the room by regulating the flow of iced water (at  $0^{\circ}$ ) from the



tap *c*. The work is constant as long as the deflection of the spot from the mirror is constant, and the heat developed is determined from the amount of overflow water at *r*. The apparatus employed absorbed about  $\frac{1}{15}$  H.P.; but one is being built to absorb 4 or 5 H.P.

#### **E. GERARD—AN ACCESSORY FOR ELECTROMETERS.**

*Lumière Electrique*, Vol. 40, No. 17, p. 191; *Bulletin de l'Institut Montefiore*, Feb., 1891.

The author suggests the use of a number of small condensers connected to a Planté revolving commutator (similar to the one in the original pattern of the Ayrton & Perry ammeter), the connections on the commutator being so arranged that the condensers are charged in parallel from the source of E.M.F. to be measured, and then joined in "cascade" or series to the electrometer. The commutator is turned a few times till the deflection of the electrometer reaches a steady value; the E.M.F. on it is then  $n$  times that of the source where  $n$  is the number of condensers used. *Vice versa*, an E.M.F. too high to be

measured may be reduced to  $1/n$ th of its value by charging  $n$  in series and discharging in parallel. The method could be used for alternating currents, but it would be necessary for the commutator to be synchronised with the alternations, which would generally be inconvenient. The author also suggests that the device might be used instead of the replenisher.

### E. BOUTY—THE SPECIFIC INDUCTIVE CAPACITY OF MICA.

(*Comptes Rendus*, Vol. 112, No. 17, p. 931.)

In well-made condensers the author finds the capacity varies but little with  $t$ , the time of charge (cf. *C. R.*, vol. cx., p. 1362), there being a difference of less than .5 per cent. between  $t = 1''$  and  $t = .1''$ , and no great difference even if  $t$  be reduced to .002''. This being so, the author wished to determine if mica, as such, had a definite S.I.C.—i.e., if there was a fixed value for  $K$  in the formula, capacity =  $\frac{Ks}{4\pi d}$ , where  $s$  = surface, and  $d$  = thickness.

$s$  was always greater than 50 cm.<sup>2</sup>, and  $d$  less than .01 cm., so that no guard ring device was necessary. The measurements were made on various sheets with  $d$  varying from 14  $\mu$  to 89  $\mu$ . The plates were coated by Martin's silvering process, the edges being very carefully cleaned, first with nitric acid, and then with water.  $K$  varied from 7.91 to 8.01, and may therefore be taken as practically constant. With tinfoil instead of silvering no such concordant results could be obtained, owing to air between the foil and the mica, which made  $K$  come out more or less low, depending on the pressure applied. There appeared to be no specific difference between the different plates as regarded absorption, but any particular plate could be made to vary enormously in this respect, according as the mica beyond the covered part was clean or dirty; and the author considers it certain that absorption is principally an effect of polarisation of foreign substances present in the surface of the mica.

### A. PEROT—ON THE SPECIFIC INDUCTIVE CAPACITY OF LIQUIDS.

(*Journal de Physique*, Vol. 10, p. 149.)

The liquids experimented on were principally benzole and "essence of petroleum." The following are the results obtained for  $K$ , the second column being the square of the index of refraction for the D line of the same samples:—

	$K$ .	$n^2$ .
Benzole ... ..	2.235	2.25
"Essence of petroleum" ...	2.170	2.11

When the liquids were quite pure and dried by sodium, it was found that  $K_0$ , the initial S.I.C., was always less than  $K_1$ , the S.I.C. after one second's charge.  $K_1$

was, however, after that time a perfectly constant value; unaffected by time of charge, charging E.M.F., or previous history. For impure liquids  $K_0$  was sometimes less and sometimes greater than  $K_1$ . For determining  $K_0$ , two electrometer needles on one stem were used, each with its own set of quadrants. The lower system was immersed in the liquid; the upper one was joined up so as to oppose the lower one, and had adjustments for its quadrants so arranged that quantitative results could be obtained. The two were then charged in parallel for a short time, and the upper system adjusted till it exactly counterbalanced the lower one, bringing the spot to zero on the scale. The two were then discharged and recharged. The spot at once went to (say) the right, showing that the torque of the lower couple was less than before, but soon returned to zero as the capacity increased to its previous value. An approximation to the value of  $K_0$  was made by adjusting so as to just suppress the throw to right and get a steadily increasing deflection to the left. The author considers that  $K_0$  for both the above liquids, and also for chloroform, is very near unity, and concludes with some curious results obtained with impure petroleum.

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# LIST OF ARTICLES

## RELATING TO

# ELECTRICITY AND MAGNETISM

Appearing in some of the principal Technical Journals during the Months  
of MAY and JUNE, 1891.

S. denotes a series of articles.      I. denotes fully illustrated.

### BATTERIES AND ACCUMULATORS.

- ROBERTS—Accumulator (I.).—*Lum. El.*, vol. 40, No. 22, p. 428.
- F. STREINTZ—Theory of Accumulators (S.).—*Ann.*, vol. 43, No. 6, p. 241.
- M. CANTOR—The Chemistry of Accumulators.—*Beibl.*, vol. 15, No. 5, p. 369.
- C. HEIM—The Life of Accumulators.—*El Zeit.*, vol. 12, No. 23, p. 295.
- F. LALANDE—New Forms of Oxide of Copper Battery.—*C. R.*, vol. 112, No. 22, p. 1253; *Lum. El.*, vol. 40, No. 23, p. 539.

### LIGHTING AND POWER.

- ANON.—The Siemens & Halske High-Tension Experiments (I.).—*El. Zeit.*, vol. 12, No. 21, p. 265.
- ANON.—Report on the above.—*Lum. El.*, vol. 40, No. 23, p. 532.
- A. FÖRDERREUTHER—The Direct-Current Machine and Alternating Currents.—*El. Zeit.*, vol. 12, No. 21, p. 267.
- SCHUCKERT—Coulomb-Meter (I.).—*El. Zeit.*, vol. 12, No. 22, p. 277.
- KORNPROBST—Coulomb-Meter.—*El. Zeit.*, vol. 12, No. 22, p. 278.
- KECCHLIN—Coulomb-Meter (I.).—*Lum. El.*, vol. 40, No. 23, p. 482.
- DESRUELLES and CHAUVIN—Coulomb-Meter (Electrolytic).—*Lum. El.*, vol. 40, No. 21, p. 380.
- ANON.—The Frager Meter, New Type (I.).—*Lum. El.*, vol. 40, No. 21, p. 377; *El. Zeit.*, vol. 12, No. 24, p. 311.
- G. RICHARD—Incandescent Lamps (S. I.).—*Lum. El.*, vol. 40, No. 20, p. 321, and *ante.*

### MAGNETISM.

- C. RAVEAU—Intense Magnetic Fields.—*Lum. El.*, vol. 40, No. 22, p. 410.
- ANON.—Ferrari's Magnetic Separator.—*Lum. El.*, vol. 40, No. 21, p. 379.

**TELEGRAPHS AND TELEPHONES.**

- BRANVILLE and ANIZAN—Artificial Lines and Cables.—*Lum. El.*, vol. 40, No. 23, p. 451.
- C. GRAWINKEL—Economy in Telegraph Batteries.—*El. Zeit.*, vol. 12, No. 20, p. 255.
- H. THOMAS and R. PICOU—The Paris-London Telephone Line.—*Bull. Soc. Int.*, vol. 8., No. 77, p. 136, No. 78, p. 188.
- P. GERMAIN—Multiple Telephone.—*C. R.*, vol. 112, No. 23, p. 1311.
- CROSS and HAYES—The Influence of the Strength of the Magnet in the Telephone Receiver.—*El. Zeit.*, vol. 12, No. 19, p. 247.
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**INSTRUMENTS AND MEASUREMENTS.**

- J. SWINBURNE—The Electrometer as a Wattmeter.—*Phil. Mag.*, vol. 31, No. 193, p. 504.
- H. WILD—A Dipping Needle with Induced Magnet.—*C. R.*, vol. 112, No. 18, p. 990.
- A. BERGET—Portable Capillary Electrometer.—*Jour. de Phys.*, vol. 10, p. 221, May, 1891.
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**STATIC AND ATMOSPHERIC ELECTRICITY.**

- ELSTER and GEITEL—Influence of the Nature of the Illuminated Surface on the Increased Loss of Charge due to Illumination.—*Ann.*, vol. 43, No. 6, p. 225.
- E. BOUTY—Dielectric Properties of Mica at a High Temperature.—*C. R.*, vol. 112, No. 23, p. 1310.
- J. WIMSHURST—Alternating and Experimental Influence Machines.—*Phil. Mag.*, vol. 31, No. 193, p. 507.
- A. HEYDWEILLER—The Spark Discharge of Condensers through Normal Air.—*Ann.*, vol. 43, No. 6, p. 310.
- S. TOLVER PRESTON—The Lightning Discharge.—*Phil. Mag.*, vol. 31, No. 192, p. 443.
- ELSTER and GEITEL—Observations on the "Hoher Sonnblick."—*Lum. El.*, vol. 40, No. 23, p. 546.
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**THEORY.**

- Dr. SVANTE ARRHENIUS—Note on Electric Conductivity of Hot Gases.—*Phil. Mag.*, vol. 31, No. 192, p. 415.
- J. BROWN—The *Rôle* of the Cation in Voltaic Combinations.—*Phil. Mag.*, vol. 31, No. 193, p. 449.
- E. BRANLY—Experiments on Electrical Conductivity (Powders, &c.).—*Bull. Soc. Int.*, vol. 8, No. 78, p. 196; *Lum. El.*, vol. 40, No. 20, p. 301 (S. I.).
- H. JAHN—Electro-magnetic Rotation of the Plane of Polarisation in Saline Solutions.—*Ann.*, vol. 43, No. 6, p. 280.

- F. KOLÁČEK—Theory of Electrical Oscillations.—*Ann.*, vol. 43, No. 6, p. 371.  
COHN and HEERWAGEN—The Period of very Rapid Electrical Oscillations.—*Ann.*, vol. 43, No. 6, p. 343.  
R. BLONDLOT—Determination of the S.I.C. of Glass by very Rapid Electrical Oscillations.—*Jour. de Phys.*, vol. 10, p. 197, May, 1891.  
J. ŠABULKA—Velocity of Electrical Waves in Long Wires (Historical).—*El. Zeit.*, vol. 12, No. 23, p. 293.
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### RAILWAY APPLIANCES.

- H. HATTEMER—Electrical Shunting Indicator.—*Lum. El.*, vol. 40, No. 22, p. 426.  
ANON.—Block Signals.—*Lum. El.*, vol. 40, No. 23, p. 483.
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### VARIOUS.

- H. PONTIÈRE—The Cost of Aluminium by Electricity.—*Lum. El.*, vol. 40 No. 19, p. 272.  
A. MAYER—Physical Properties of Ebonite.—*Lum. El.*, vol. 40, No. 20, p. 334.  
G. RICHARD—Phonographs (S. I.).—*Lum. El.*, vol. 40, No. 23, pp. 512, &c.  
J. and F. RICHARD—Electric Repeating Apparatus.—*Lum. El.*, vol. 40, No. 23, p. 485.  
C. CARRÉ—Electricity applied to Organs (S. I.).—*Lum. El.*, vol. 40, No. 20, p. 291-341, *et seq.*  
CHASSAGNY and ABRAHAM—Thermo-electrical Researches.—*C. R.*, vol. 112 No. 121, p. 1198.
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# NOTICE.

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1. The Society's Library is open to members of all Scientific Bodies, and (on application to the Secretary) to the Public generally.
2. The Library is open (except from the 14th August to the 16th September) daily between the hours of 11.0 a.m. and 8.0 p.m., except on Thursdays, and on Saturdays, when it closes at 2.0 p.m.

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*An Index, compiled by the late Librarian, to the first ten volumes of the Journal can be had on application to the Secretary, or to Messrs. E. and F. N. Spon, 125, Strand, W.C. Price Two Shillings and Sixpence.*

The Institution is not, as a body, responsible for the opinions expressed by individual authors or speakers.

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# JOURNAL

OF THE

## Institution of Electrical Engineers.

*Founded 1871. Incorporated 1883.*

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VOL. XX.

1891.

No. 95.

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The Two Hundred and Twenty-sixth Ordinary General Meeting of the Institution was held at the Institution of Civil Engineers, 25, Great George Street, Westminster, on Thursday evening, November 12th, 1891—Professor WILLIAM CROOKES, F.R.S., President, in the Chair.

The minutes of the Ordinary General Meeting held on May 21st, 1891, were read and confirmed.

The names of new candidates for election into the Institution were announced and ordered to be suspended.

The following transfers were announced as having been approved by the Council:—

From the class of Associates to that of Members—

Professor W. F. Barrett.  
William Hamilton Blakeney.  
Charles Butler Clay.  
W. A. Coulson.  
Herbert J. Dowsing.  
Edward Eugène-Brown.  
William Gibbings.  
Ernest F. J. Hewlett.

Charles Hortsek.  
Frank Lumley.  
William Henry Patchell.  
Douglas L. Wells.  
George Wilkinson.  
Arthur C. F. Webb.  
George R. Webb.  
H. C. Wilson.

From the class of Students to that of Associates—

Archibald John French.  
Thomas Leopold Horn.  
Edward Alfred Horton.  
W. B. Lloyd.  
Hubert H. Nalder.

J. K. Stothert.  
John Pratt Sleigh.  
George B. Winter.  
Richard Wightman.

The  
President.

The PRESIDENT: I have to inform you that a new method of balloting for members will take place from this date. You are all aware that under the present system it is rather a difficult thing to carry through properly. The ballot takes place after the meeting, when many members have left and the remainder are anxious to leave, and it is consequently no easy matter to get any but a few to vote. Another reason is that we have not a sufficient number of ballot boxes to meet the requirements, there being frequently so many candidates that the boxes have to be used perhaps three times over, thus rendering the process very tedious. In future, therefore, the same system as that practised by the Institution of Civil Engineers will be followed. Members and Associates, when signing the attendance book, will be furnished with voting papers containing a list of the names and addresses of all the candidates, and ballot boxes will be placed at the entrance of the Lecture Hall. The ballot will close at 8.30, and scrutineers will be appointed to examine the papers deposited and report the result. This arrangement will come into force at the next meeting.

I have now the pleasure to announce that the Council have elected Mr. Jacob Brett an Honorary Member of the Institution, in recognition of the valuable and important services rendered by him to Telegraphic Science. (Applause.)

Donations to the Library were announced as having been received from the Astronomer Royal; Mr. J. W. Cockrill; Council of Manchester Technical School; Messrs. W. Dawson & Sons; Mr. A. Hartleben; Messrs. Longmans & Co.; Mr. Jacob Brett, Honorary Member; Sir William Thomson, Past-President; Don Felix Garay, Professor E. Gerard, and Dr. K. Zetzsche, Foreign Members; Professor A. Jamieson, Mr. A. F. Matveieff, Mr. W.

Perren Maycock, and Professor H. Robinson, Members; and Mr. C. Langdon Davies, Associate; to whom the thanks of the meeting were duly accorded.

The PRESIDENT: I have now to make known a further valuable <sup>The President.</sup> donation just made by Mr. Latimer Clark, Past-President, consisting of seven volumes of letters and papers, formerly in the possession of the late Sir William Fothergill Cooke, relating to the introduction of the electric telegraph in England by himself and his partner, the late Sir Charles Wheatstone. These very interesting volumes are on the table. I propose a special vote of thanks to Mr. Latimer Clark for this most valuable contribution to our archives.

The motion was carried by acclamation.

The PRESIDENT: I have also the pleasure of announcing the presentation by Miss Alice Bolton of the portrait in oils, painted by herself, of Mr. Jacob Brett, our newly elected Honorary Member, which you see before you. Looking at that portrait carries me back to a time more distant than I thought. Mr. Jacob Brett was the first pioneer of submarine telegraphy. I well remember, in 1851, the great excitement produced by the announcement made of an attempt to get messages transmitted under the Channel by a submarine cable. We have become so accustomed to Atlantic telegraphs and long submarine lines by the score, that we are apt to forget the excitement occasioned by the laying of the first cable. Most people who did not know so much about it as the pioneers themselves, said that to lay a cable of such a length was impossible—it could not be done—or, at all events, if it were laid, it would not speak; and when the announcement came that it was laid successfully, and did speak, and continued to live, it produced, I think, more excitement than anything of the kind that has occurred since. I can very well remember it, although I was only a student. I should say that this was an admirable portrait. We had hoped to have the pleasure of seeing the artist, Miss Bolton, herself here to-night, but for some reason she did not like to face such an assemblage of eminent men. I now beg to propose a special vote of thanks to Miss Alice Bolton for presenting this portrait to the Institution, and also an

The  
President.

expression of our thanks for the great kindness she and her family have shown Mr. Brett in his declining years.

Mr. Clark.

Mr. LATIMER CLARK: I have great pleasure in seconding this motion; but before referring further to the subject I feel bound to return my thanks for the kind way in which you have accepted from me the letters, documents, and papers of the late Sir William Fothergill Cooke, which I have had the honour to present to the Institution. Anyone who looks into these letters will find their perusal very interesting, and I can vouch for the fact that they are historically extremely valuable, because they relate to the history of the introduction of the electric telegraph into this country at a date so early that it is not possible that anything earlier can be discovered. We owe it to the business aptitude and energy of Sir William Cooke, assisted by his partner, Wheatstone, that the electric telegraph was introduced on the railways of this country at a very early date, and before it was taken up by other countries; so that Great Britain will for all time to come claim the honour of having first introduced the electric telegraph into practical use, as it can also claim the first invention of electric telegraphy through the celebrated letter of "C. M." in the *Scots Magazine* of 1753.

Although I knew Sir William Cooke extremely well, and for a great number of years, it was a long time before I could induce him to entrust to me the valuable papers he had in his possession. For many years I used, on every opportunity, to urge on him that they should be deposited in some public institution, and it was only some ten years ago that he at last consented to place them in my hands, as part of a large collection of works which I had been accumulating, and which I assured him would sooner or later find their way to some public library. When I did obtain possession of them, a great many were found missing, in spite of every search he could make. They now comprise seven volumes, containing all kinds of documents of a legal or business nature and a great many private letters. Perhaps the most interesting part of the collection is a series of private letters to his mother and his brother detailing the inception and early development of his ideas. He was most affectionately attached to his mother, and his letters to her abound in expressions of filial love which are quite touching. They were

written in the old days of high postage rates, and, as was customary *Mr. Clark.* in those days, they were crossed and recrossed in such a way that it is difficult to read them, and would be still more difficult to find space for an additional 20 words in any of them.

Our Honorary Solicitor, Mr. George Bristow, has very considerably made a copy of all those portions which relate to the electric telegraph, and this will be found in a separate volume. He has done this not only with a view to their more easy perusal, but he was impressed with their historic value, and he kindly undertook this duty with a view to their safe preservation from fire or other accident, deeming it unsafe that only one copy should be in existence. I will venture to read an extract from the first letter to his mother, dated April 5th, 1836, because it is always interesting to trace the first movement in a path from which such mighty results are afterwards destined to emanate.

“HEIDELBERG, *April 5th*, 1836.

“MY DEAREST MOTHER,—You must know that for some  
“weeks past I have been deeply engaged in the construction of  
“an instrument which I believe may prove of sufficient import-  
“ance, should I succeed in bringing it to practical perfection, to  
“merit a visit to London. Determined to satisfy myself on the  
“working of the machinery before I went any further, I pre-  
“pared to make a model, and, being unable to obtain the  
“requisites at Heidelberg, I sought them at Frankfort. Whilst  
“completing the model of my original plan, others, on entirely  
“fresh systems, suggested themselves, and I have at length  
“succeeded in combining the utile of each; but the mechanism  
“requires a more delicate hand than mine to execute, or, rather,  
“instruments which I do not possess. These I can readily have  
“made for me in London, and, by the aid of a lathe, I shall be able  
“to adapt the several parts, which I shall have made by different  
“mechanicians, for secrecy sake. Should I succeed, it may be the  
“means of putting some hundred pounds in my pocket. As it  
“is a subject on which I was profoundly ignorant till my atten-  
“tion was casually attracted to it the other day, I do not know  
“what others may have done in the same way; this can best be  
“learned in London. You see I am very mysterious at present,

Mr. Clark.

“and think it very prudent to continue so; nevertheless to you, “dearest mother, if it were your wish, my plan and instrument “should be explained now, though I think, without better “drawings than I could make, you would scarcely comprehend “me. As I do not wish my motives for revisiting London to be “generally known, you had better, in mentioning it to any “friends at Berne, state that private business requires my “presence, and allow them to ascribe to modelling, or what they “please, the sudden change of my plans.”

This is a sample of the correspondence, which goes on for a great many years. But, besides these, the collection comprises autograph letters from several very eminent persons, and many papers connected with the celebrated arbitration between himself and Sir Charles Wheatstone, and with the introduction of the telegraph and the formation of the Electric Telegraph Company, which was the first public company established for the purpose of telegraphic communication, and which opened its doors to the public on the 1st January, 1848, at which time about 1,500 miles of telegraph were in existence.

Life is proverbially uncertain, and I have therefore had very great pleasure in confiding these papers and documents to the care of the Institution of Electrical Engineers, and I thank you sincerely for the kind way in which you have accepted them.

I have now the pleasure of seconding the vote of thanks which has been proposed to Miss Alice Bolton, of 49, Blomfield Road, Maida Vale, for her presentation to this Institution of the very artistic and excellent portrait of Mr. Jacob Brett which we see before us. Its artistic merits are very obvious to us all, and I can testify to its being an excellent likeness, as I have had the pleasure and privilege of knowing Mr. Brett for a great many years. It represents him not so much as he is to-day, but rather as he was some four or five years since, when the portrait was originally painted. Mr. Jacob Brett is now 83 years of age, and I grieve to say he lies on a sick bed through an accident which occurred to him some time ago in getting out of a vehicle; one of his legs was injured, and, as the wound has reached the surface of the bone, it is extremely

difficult to heal. It is a great pleasure to me to know that Mr. Clark.  
Mr. Jacob Brett has been elected one of the very few Honorary Members of this Institution, for I feel that in conferring honour on Mr. Brett we have, perhaps unwittingly, conferred an honour upon ourselves.

I do not think it probable that many of those whom I see present with us this evening can be fully aware of the very important part which Mr. Jacob Brett and his brother, Mr. John Watkins Brett, have played in connection with the history of the introduction of the electric telegraph, and especially the submarine electric telegraph, into the country and into the world. That connection dates back to a past generation, and is much deeper and more important than the world generally is aware of. His brother, Mr. J. W. Brett, and Mr. Brett were close friends and partners, and worked together in all their undertakings. His brother had a considerable fortune, which he was always ready to lavish on the advancement of submarine telegraphy. They commenced their public labours in 1845, and it is through their energy and devotion to the subject that we as Englishmen can claim for ourselves the distinction of having been the pioneers in the introduction of submarine telegraphy, as we can equally claim to have been the first nation to invent the electric telegraph, and the first nation to introduce it into practical working. In order to be able to appreciate the early date at which they began their labours, I would remind you that in 1837 Cooke and Wheatstone exhibited the private working of their telegraph from Euston to Camden Town, and the public then for the first time became aware of the possibility of the introduction of the electric telegraph as a factor in daily life. During the ensuing years, Sir W. Cooke, by his great business energy, succeeded in establishing telegraphic lines on many of our railways, as, for example, the Norwich and Yarmouth line, the Dalkey atmospheric line, the Northampton and Peterborough, the South Western, the South Eastern, the Great Western, and others. But on the 1st of January, 1845, an event occurred which startled the public mind and contributed most powerfully to the advancement of

Mr. Clark.

the electric telegraph. On this date occurred the notorious case of the murder of a woman at Slough by the Quaker John Tawell, and his arrest was effected by the telegraph. After he had left by train for Paddington, a description of the murderer was forwarded by telegraph, and on his arrival he was seen to enter an omnibus. A detective mounted on the roof, and, after watching him through several streets in the City, followed him into a small eating-house in an obscure and narrow alley, and seated himself opposite to him. Having satisfied himself by observation that he was in the presence of the right man, he suddenly accosted him with the question, "Haven't you just come from Slough?" Tawell's astonishment and his haggard looks at once betrayed his guilt, and he was eventually condemned and hanged for the crime. This episode occurred in 1845, and made a great impression on the public mind, and I have no doubt had its influence on the action of the two brothers Brett, for, on the 16th of June of that year, they registered "The General Oceanic Telegraph Company." I shall have to refer later on to this company, but for the moment I only call attention to its date, and to the fact that "The Electric Telegraph Company," the well-known pioneer telegraph company of Great Britain, was not registered until the 2nd of September in the same year.

On the 23rd July, 1845, the two brothers laid before Sir Robert Peel and the Government their plans for uniting Dublin Castle with Downing Street, and also for a general system of oceanic and subterranean electric telegraphs to include the United Kingdom and the Colonies, together with the establishment of a system of postal telegraphs throughout Great Britain. That letter was printed in type by an electrical type machine patented by the Bretts, which was exhibited at 29, Parliament Street, and I well remember seeing it at that time. Copies of the letter were extensively distributed, and I believe that the instruments themselves are still in the possession of Mr. Jacob Brett. In the following year—1846—they applied to the French Government for a concession for a cable to be laid at their own expense from England to France, and in 1847 this concession was granted to Messrs. Brett by His Majesty Louis Philippe, and in 1849 was further confirmed by

Louis Napoleon, the President of the French Republic. They also Mr. Clark. applied in this year for a Belgian concession, which was granted to them in 1852.

In 1850 they laid down their first submarine line, the well-known single wire of gutta-percha, which was successfully completed on the 28th of August, just in time to save their concession. That wire, as we well know, was so slight that it failed almost immediately, but it was followed in 1851 by a permanent cable of a very different character, which, subject to repairs, was working in portions for 20 years or more ; in fact, no record exists of the date of renewal of the last remaining portions of it. This cable contained four conductors, and was armoured with stout iron wires in the usual manner ; in fact, it formed the type for all future submarine cables. The success of this line enabled them to form, in 1852, "The Submarine Telegraph Company," Mr. J. W. Brett himself being one of the directors, which proved one of the most prosperous and successful of cable companies, and has only recently passed into the hands of the British Government.

In 1853 they laid the first Belgian line, and formed "The Mediterranean Electric Telegraph Company," followed by cables to Corsica, Sardinia, Algiers, and many other places, while they were incessantly active in pushing forward their great scheme for a line of cables across the Mediterranean to Egypt, the Red Sea, India, and Australia. Mr. J. W. Brett was also a director of "The British and Irish Magnetic Telegraph Company," which, in combination with others, formed so powerful a rival to the old Electric Telegraph Company.

Now it must ever be a source of surprise that, with such records as these, the name of Brett should have been omitted or forgotten in 1866, when the somewhat lavish distribution of honours and titles was made in consideration of the splendid success of the Atlantic undertaking ; and that surprise will not be diminished when I proceed to show you that Mr. J. W. Brett was himself the originator, and one of the earliest and strongest supporters, of that project.

I have said that on the 16th June, 1845, Mr. Jacob Brett had registered at the Joint Stock Companies' Office "The General

Mr. Clark.

“Oceanic Telegraph Company,” the first telegraphic undertaking ever registered. I have seen the original certificate and receipt. But what is very curious is that even at that early date the application goes on to say “Object specified—To form a connecting mode of communication by telegraphic means from the British Islands and across the Atlantic Ocean to Nova Scotia and the Canadas, the Colonies, and Continental kingdoms.” So that we are met by the extraordinary fact that the first telegraphic company ever registered in this country was formed, not with the object of erecting telegraphs on land, but for the purpose of laying a submarine cable across the Atlantic Ocean, and this at a period when the world at large had not even heard of submarine telegraphy. Later on, in 1855, we find Mr. J. W. Brett associated with Peter Cooper, Cyrus Field, Dudley Field, Professor Morse, and other prominent names, in founding “The New York, Newfoundland, and London Telegraph Company,” and I have seen the receipt for the £3,000 which he contributed in 1856 as his share to the common fund. In October, 1856, we find the “Memorandum of Association of the Atlantic Telegraph Company,” with a capital of £300,000 in 300 shares of £1,000 each and again we find Mr. J. W. Brett’s name at the head of the list with a subscription of £25,000, followed by the subscription of a like sum by Cyrus W. Field; and subsequently we find his name at the head of the list of directors of the Atlantic Telegraph Company, which was at first unsuccessful.

It is most difficult to understand how such splendid services in the cause of Atlantic telegraphy could have gone unrequited and unnoticed. One can only conjecture that he must have impoverished his fortune by his too great liberality in his efforts to advance the cause of submarine telegraphy, so that when the final victory was won, and the hour of triumph and reward came, he was unable to take his place in front, and was left aside unnoticed and forgotten.

I feel very much gratified that this Institute should possess a portrait of the only survivor of these two brothers, whose names will go down to posterity as those of the fathers of submarine telegraphy, and I have, therefore, the greatest pleasure in

seconding the vote of thanks to Miss Alice Bolton. I can only Mr. Clark. regret that the very artistic portrait which she has presented to us this day cannot be seen by a wider circle, for it could not fail to add largely to her professional reputation. We also owe a debt of gratitude to her and her mother for the affectionate care with which they have assisted Mr. Brett during many long years of public neglect. As for Mr. Brett himself, I regret to say that he is in extremely straitened circumstances. He has for many years subsisted entirely on a pension of £100 a year which was granted to him from the Civil Service Fund, and he has no other means of subsistence. It will ever be regarded as a matter of wonder to future generations that the rulers and merchant princes of the Victorian era, who have derived such splendid benefits from submarine telegraphy, should have permitted two of the noblest of the pioneers of electrical science to thus pass away unnoticed and almost unknown.

Mr. Brett is, however, fortunately, a man of happy and cheerful disposition, and feels amply repaid by the knowledge that the subject to which he devoted his early life has now become of almost infinite importance to mankind; that by day and night myriads of signals are ceaselessly flashing through every ocean, and bearing their priceless messages of love or of commerce, of peace or of war. It is a pleasure to feel that when he shall be called away to his last rest, he will be sustained by the proud consciousness that he has been instrumental in conferring greater benefits on his country and on the world than any other man now living.

The PRESIDENT: I think, in putting this motion, we ought The President. to also thank Mr. Latimer Clark for the very admirable account which he has given of the works of the eminent man who has, we must feel after hearing that account, not met with the appreciation that he deserved.

The motion was carried by acclamation.

The following paper was then read:—

## DESCRIPTION OF THE STANDARD VOLT- AND AMPERE-METER USED AT THE FERRY WORKS, THAMES DITTON.

By Captain H. R. SANKEY (late R.E.), Member, and  
F. V. ANDERSEN, Associate.

### 1. INTRODUCTORY.

It is well known to many electricians that Messrs. Willans & Robinson have established at their works at Thames Ditton a complete arrangement for measuring accurately the steam consumption of their engines. To obtain the efficiency of combined sets of engines and dynamos, measurements must of course also be made of the electric current and pressure, and it was thought that even the best ammeters and voltmeters in the market could not be relied upon to give the same degree of accuracy as was obtained in the steam-consumption measurements. At any rate, all such instruments laboured under the disadvantage that they were calibrated elsewhere, so that the results were dependent upon other people's measurements to a degree which was thought to be undesirable.

After a series of investigations—commenced as early as 1887—into the question of the class of apparatus which would be most likely to give satisfactory results under the special circumstances, a scheme was drawn up in 1889 for a complete apparatus for measuring currents, differences of potential, and resistances, and the various instruments were designed and ordered. It was not, however, until the beginning of the present year that time and opportunity were found to have the apparatus put to practical use, various additions and improvements being introduced as the work proceeded. It is well to mention at once that the results obtained with this apparatus are in practical agreement with those obtained with the Siemens dynamometers previously in use.

As will be seen in the sequel, no new principles have been

introduced in electrical measurements with the present apparatus. It was thought, however, that a complete set of workshop instruments, fulfilling the conditions for high accuracy, and yet possessing the simplicity required for everyday use in an engineer's shop, would be found of sufficient interest to electrical engineers to justify a short account of it being brought before the Institution.

The more important conditions imposed in designing the apparatus were that—

- (1.) All measurements made must be capable of verification at any time by a direct comparison of certain resistance coils with a standard ohm, and of a constant potential difference with a standard cell; and that the arrangements should allow of these comparisons being made quickly for the satisfaction of those interested in the accuracy of the measurements.
- (2.) Volts and amperes must be indicated by direct deflection on a scale, and no balancing operation be required from the observer by torsion arrangements or otherwise; and the value of the readings must not be influenced by the working of dynamos or the moving of large masses of iron within a few yards' distance.
- (3.) The ammeter as well as the voltmeter must be left in circuit permanently during trials of any desired length.
- (4.) The errors to which the measurements are liable should not exceed 1-5th per cent.
- (5.) The range, such as to allow of measurements being made of pressure from 1-1,000th part of a volt to 700 volts, and of current from 1-40th of an ampere to 1,100 amperes.

## 2. APPARATUS REQUIRED.

The complete apparatus comprises the following :—

### I. *For measuring amperes :*

- 1 D'Arsonval galvanometer.

- 1 high-resistance box, adjustable by the unit, for use in series with the galvanometer.
- 1 rheostat of platinoid strips for carrying the main current (called resistance B).

*II. For measuring volts:*

- 1 D'Arsonval galvanometer.
- 1 high-resistance box, adjustable as above, for use in series with the galvanometer. A Wheatstone bridge box is used for this purpose.
- 3 potentiometer rheostats, 250 ohms each, made of platinoid wire on screwed slate rods, carrying capacity up to 1 ampere.

*II. For calibration:*

- 1 Clark standard cell containing two elements.
- 1 standard ohm.
- 1 standard rheostat of 5 platinoid strips, arranged for coupling in parallel or series, each strip capable of carrying 10 amperes (called resistance A).
- 1 mercury rheostat, capacity 50 amperes.
- 1 secondary battery, 3 large cells (15L E.P.S.).
- 1 key with double set of contacts, for use with standard cell.
- 1 resistance coil, 10,000 ohms, for use with same.
- 2  $\frac{1}{3}$  shunts for galvanometers.

*IV. General.*

Leclanché cells for bridge test, keys, ebonite pillars for connections, &c.

The method of using the apparatus for actual measurements will first be described, after which the calibration of the galvanometers and the verification of the various resistances will be discussed.

### 3. MEASURING AMPERES.

The apparatus used for this purpose is shown on Fig. 1, with the connections in full lines. On the same figure is drawn the

voltmeter arrangement, in order to give the general arrangement of the whole set of instruments; the circuits of the voltmeter arrangement are in dotted lines. It will be seen from Fig. 1 that for current measurements the method of measuring the fall of potential across a known resistance has been adopted. The measurement of this resistance—*i.e.*, the resistance of rheostat B, which, of course, must be made with great accuracy—could not be taken satisfactorily by a Wheatstone bridge, owing to its small value (about  $\cdot 001$  ohm); the method adopted for this purpose will be described further on.

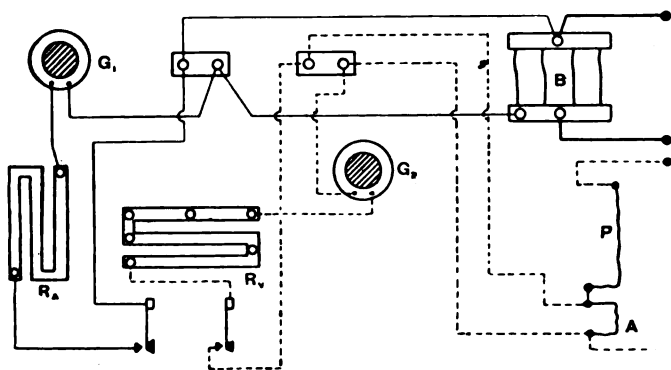


FIG. 1.

The scale of the D'Arsonval galvanometer,  $G_1$ , has, in addition to the ordinary millimetre scale, another scale graduated to read up to 1,100 amperes, on which the divisions are proportional to the corresponding currents through the galvanometer, the relation or constant connecting the deflections with this current being known from the calibration of the galvanometer.

By modifying the resistance,  $R_A$ , used in series with the galvanometer, the constant of the ammeter can be modified to suit the number of amperes to be measured. If

$k$  = current through  $G$  per unit deflection on ampere scale;

$K$  = " "  $B$  " " " " "

$B$  = resistance of  $B$ ;

$G$  = " of galvanometer;

$R_A$  = " in series with galvanometer:

then for any deflection,  $D$ , on the ampere scale,

$$(G + R) k D = K D B.$$

Hence, 
$$R_A = K \frac{B}{k} - G \quad \dots \quad \dots \quad \dots \quad (1)$$

In the present case we have,

$B = .0010882$  legal ohm at standard temperature,  $17^\circ \text{C}$ ;

$G = 429$  " " " " "

$k = .06 \times 10^{-6}$  amperes;

and therefore the resistance,  $R$ , to be used in series with the galvanometer to obtain different ammeter constants  $= K \times 18,137 - 429$  legal ohms.

Table I. gives the constants in use, and the corresponding values of  $R_A$ .

Table I.

Ammeter Constant, K.	$R_A$ in Legal Ohms.	Temp. Correction (add to $R_A$ ).	REMARKS.
1	17,708	$4.17 \text{ } T + 1.57(17 - t)$	$\left\{ \begin{array}{l} T = \text{rise of temp. in B due to current} \\ = \frac{2}{100,000} C^2 \text{ degrees} \\ \text{centigrade; } t = \text{temp. in } ^\circ \text{ cent. of galvanometer.} \end{array} \right.$
0.5	8,639	$2.09 \text{ } T + \text{do.}$	
0.1	1,385	$0.417 \text{ } T + \text{do.}$	
0.025	24.4	$0.104 \text{ } T + \text{do.}$	

The following are the corrections for temperature which must be made in  $R_A$  in order to get the accuracy desired :—

(1.) For the galvanometer, at the rate of  $1.57$  ohms per  $^\circ \text{C}$ . variation from the standard temperature—in this case  $17^\circ \text{C}$ .

(2.) For the rise of temperature in  $B$  due to the current at the rate of  $.023$  per cent. per  $^\circ \text{C}$ .

The amount of resistance per degree is given in the third column of the table; and the constant for calculating, from the current, the rise of temperature in  $B$ , as determined by experiment, is given in the fourth column.

When the main rheostat, B, is made from the same metal and placed in the same room as the resistance box  $R_A$ , the periodic temperature variations in the atmosphere do not affect the measurements of the current through the increase of the resistances of these two instruments, as the increase in one balances that in the other.\*

For ordinary work the readings on the scale are amply accurate, as they can be depended upon to be within .3 per cent. of error.

For very accurate trials, however, the scale is used only to ensure the practical steadiness of the load, and for taking readings at short intervals in order to obtain the mean reading. The current corresponding to this mean reading is then determined by direct comparison with the standard ohm and cell.

This determination of the current is made by substituting in the galvanometer circuit for the P.D. on the terminals of B, an E.M.F. equal to that of the standard cell, shunting the galvanometer, and then adjusting  $R_A$  until the deflection in question is reproduced. If

$C$  = the current to be determined ;

$G$  = „ resistance of the galvanometer ;

$R_1$  = „ „ in series with  $G$  when the standard cell is in circuit ;

$R_A$  = „ resistance in series with  $G$  when B is in circuit ;

$e$  = „ E.M.F. of standard cell ;

and if the shunt is of a multiplying power =  $m$  : then

$$C = \frac{(R_A + G) e}{(m R_1 + G) \cdot B} \quad \dots \quad (2)$$

#### 4. MEASURING VOLTS.

The apparatus for this purpose is shown on Fig. 1 (with connections in dotted lines) in a diagrammatic form. It consists of a D'Arsonval galvanometer,  $G_s$ , in series with a resistance,  $R_v$ , together with a rheostat, P, connected with the difference

\* Thus, in the case of constant 1, the  $4 \cdot 17$  is  $\frac{.023}{100} (R_A + G) = \frac{.023}{100} (17,708 + 429)$ .

of potential leads—for instance, with the terminals of a dynamo.

The scale of the galvanometer has a second graduation constructed in accordance with a calibration, so as to have its divisions proportional to the corresponding *currents* through the galvanometer. This proportional scale is divided up to 175 volts, and unit deflection on the volt scale is produced by a current through the galvanometer of 1 micro-ampere.

In measuring P.D.'s smaller than 1.75 volts, the rheostat P is not used, but the P.D. to be measured is directly connected into the circuit of  $G_2$ .

When measuring P.D.'s higher than 1.75 volts, the P.D. to be measured is connected across the rheostat P, of which a small section—"A" on Fig. 1—is connected with the circuit of  $G_2$ . This section "A" is so adjusted as to be exactly 1-100th part of the total resistance of P + the dynamo leads, which sum is = 250 ohms.

The ratio  $\frac{a}{P + \text{leads}}$  is called the potentiometer factor.

When it is required to measure high differences of potential, a second and even a third rheostat,  $P_1$  and  $P_2$ , each of 250 ohms, can be joined in series with P, and altering the potentiometer factor from 1-100th to 1-200th or 1-300th. Further alterations can, of course, be made in the voltmeter constant by varying the resistance  $R_v$ , as in the case of the ammeter.

The potentiometer factor is evidently independent of the temperature of the rheostats P. The temperature correction required in  $R_v$  is simply .023 per cent. of  $R_v^*$  + .388 per cent. of  $G$  per ° C. of variation. It will be seen that for the ordinary ranges the resistance of the galvanometer—about 435 ohms—is very small compared with  $R_v$ , and that therefore the errors in the measurements due to unavoidable small errors in the estimation of the temperature of  $R_v$  are quite insignificant. If

$k$  = current through  $G$  per unit deflection on volt scale ;

$K$  = volts to be measured        "        "        "

$f$  = potentiometer factor ;

---

\* The coils being made of platinoid wire.

$G$  = resistance of galvanometer ;

$R_v$  = „ in series with galvanometer :

then, for any deflection,  $D$ , on the volt scale,

$$(G + R) k D = f K D ;$$

$$\therefore R = \frac{f}{k} K - G \dots \dots (3)$$

In the present case,

$k = 1$  micro-ampere =  $1 \times 10^{-6}$  amperes ;

$G = 435$  ohms (legal) at standard temperature ;

therefore the resistance  $R$  to be used in series with the galvanometer to obtain different voltmeter constants =  $f K \times 10^6 - 435$ .

Table II. contains, in column 1, the constants used with the instrument ; in column 2, the corresponding values of  $R_v$  ; and in 3, the value of the temperature correction in ohms, per ° C. deviation from the standard temperature—17° C.

Table II.

Voltmeter Constant, K.	$R_v$ in Legal Ohms.	Correction per ° C. from 17° C. - or +.	REMARKS.
0.0005	60	1.68	} Direct connection.
0.001	555	1.80	
0.005	4,517	2.71	
0.01	9,469	3.85	
0.05	60	1.68	} 1 potentiometer rheostat.
0.1	555	1.80	
0.5	4,517	2.71	
1	9,469	3.85	
2	„	„	2 ditto in series.
3	„	„	3 „ „
4	12,773	4.61	3 „ „

NOTE.—The resistance  $R_v$  by formula 3 is of course in true ohms ; the amounts of  $R_v$  in Table II. are given in legal ohms, as the resistance boxes available were adjusted in legal ohms. For the purpose of reduction, 1 legal ohm has been taken = .9977 ohm. The figures have further been reduced about 1.2 per cent. to balance a change of constant.

The strongest current which at any time can pass through the galvanometer circuit is the current which deflects to the end of the scale, or .000175 ampere. The heating caused by this current in the galvanometer, as well as in the coils of the resistance box, is quite inappreciable, and the galvanometer may therefore be left "on" for any length of time.

For everyday work the scale is used, the readings being well within .3 per cent. of error. For accurate work, however, the scale is only used to obtain the mean reading (as in the case of current). The mean reading is afterwards calibrated by placing across the terminals of  $G$  and a resistance,  $R_1$ , an E.M.F. equal to that of the standard cell, shunting the galvanometer, and adjusting the resistance  $R_1$  until the mean deflection is reproduced. Then, if

$E$  = the P.D. required;

$r$  = total resistance of potentiometer rheostat plus dynamo leads;

$r_a$  = section of this resistance tapped to galvanometer;

$R_1$  = resistance in the galvanometer circuit required to reproduce the mean deflection;

$m$  = multiplying power of shunt when standard cell is used;

$e$  = E.M.F. of standard cell corrected for temperature:

we have,

$$E = \frac{r}{r_a} \cdot \frac{R_v + G}{m R_1 + G} e \quad \dots \quad (4)$$

## 5. MEASURING RESISTANCES.

With the addition of a few Leclanché cells, with key, and a short-circuit key for the galvanometer, the apparatus contains everything required for making Wheatstone bridge tests with good accuracy.

Very small resistances, such as the resistance of a dynamo armature or of a brush contact, are, however, taken by passing a current through the resistance, reading simultaneously the amperes passing through it and the P.D. at its ends. The numerous constants given for the ammeter and voltmeter on Tables I. and II. enable the tester to get accurate measurements

on this plan with currents differing in strength according to circumstances. The following is an example of a test of the resistance of an armature with two different currents:—

Amperes through Armature.	Volts between Brushes.	Resistance in Ohms.
25·9	·203	·00784
47·1	·370	·00786

Insulation resistance can be measured by the deflectional method, using the E.M.F. of any available dynamo; and as the value of the current through the galvanometer is read direct, the time usually spent in determining the constant of the galvanometer is saved.

#### Example:

The E.M.F. of a dynamo running excited without load was measured on the voltmeter galvanometer,  $G_2$ , in the ordinary way, and found = 103 volts. The circuit from this dynamo was then opened, and again closed by joining one end of it to the shaft, and the other end to the commutator of an armature the insulation of which was to be tested. The insulating material of the dynamo armature thus formed part of the circuit, which was made *direct* through  $G_2$ —the potentiometer rheostat having been disconnected. The reading on the volt scale was now 89·5; *i.e.*, a current of 89·5 micro-amperes was passing through the circuit.

The total resistance of the circuit was therefore

$$\frac{103 \times 10^6}{89 \cdot 5} \omega = 1 \cdot 151 \text{ megohms,}$$

consisting of the insulation resistance + a few hundred ohms resistance of  $G_2$  and leads.

The experiment was made in the manner described because there did not happen to be at the moment two sets of leads available, so one galvanometer ( $G_2$ ) had to be used. Otherwise the best way is to measure the volts by  $G_2$  and the current by  $G^1$ .

## 6. DESCRIPTION AND CALIBRATION OF INSTRUMENTS.

*Galvanometers.*—As has been already mentioned, these are of the D'Arsonval type, which was chosen on account of its dead-beatness, and more particularly its independence of variations

in the surrounding magnetic field. It is hardly too much to say that a system such as that described in this paper, when used in a workshop, depends on the D'Arsonval galvanometer for its existence. The galvanometers are mounted on a stone supported on thick india-rubber rings—an arrangement which has been found to a large extent to stop the transmission of vibrations. The scales are at a distance of 5 feet from the galvanometers.

*Calibration of Galvanometer.*—Fig. 2 shows the connections and general arrangement. A current is established by means of

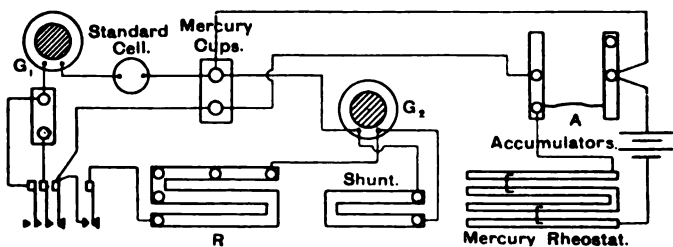


FIG. 2.

two or three secondary cells through two suitable rheostats, one having a fixed and the other a variable resistance, the latter made of mercury placed in grooves of a wooden trough with two copper slides. The strength of this current is varied by adjusting the mercury rheostat until the fall of potential across the fixed resistance—"A" on the diagram, Fig. 2—the terminals of which are connected with two mercury cups on the instrument table, is equal to the E.M.F. of the standard cell. The balance is effected by placing a standard cell joined in series with a second galvanometer in opposition to the P.D. between the two mercury cups, and adjusting the mercury resistance until no deflection is obtained on the second galvanometer. The galvanometer to be calibrated forms, with an adjustable resistance box in series, and generally with a shunt across its terminals, a circuit between the same mercury cups. When no deflection is obtained on the second galvanometer, the fall of potential across the circuit of the galvanometer under calibration is equal to the E.M.F. of the standard cell. Clearly, therefore, the current in the galvanometer can be easily found by means of the formula,

$$c = \frac{e}{m R + G} \dots \dots \dots (5)$$

where  $c$  = current through galvanometer ;

$m$  = multiplying power of shunt on  $G$  ;

$R$  = resistance in series with  $G$  ;

$G$  = „ of galvanometer.

As a practical matter, the values of  $R$  based on the use of a certain shunt are calculated beforehand, so that the current shall be 10, 20, 30 . . . micro-amperes ; and the observations are noted as shown on Table III., which gives the calibration of galvanometer No. 1588, taken on the 22nd April last.

In the circuit of the standard cell a key is used which has double contacts, and is connected with a high resistance (about 10,000 ohms) in such a manner that, when the key is depressed half-way, this resistance prevents an injurious current from passing through the cell, whilst, when the key is completely depressed, after the balance has been very nearly established, the resistance is short-circuited, and so the full sensitiveness of the arrangement obtained.

When the P.D. between the mercury cups is equal to the E.M.F. of the standard cell no current passes through the second galvanometer, the spot of which will move visibly for a current of 1-ten-millionth of an ampere. As the internal resistance of the standard cell plus the resistance of the galvanometer does not exceed 1,600 ohms, a visible deflection is obtained, one way or the other, if the balance is at fault to the extent of  $\frac{1}{10^7} \times 1,600$ , or .00016 volts, which is about .011 per cent. of the E.M.F. of the standard cell.

The mercury varying but little in resistance by temperature, and giving perfect joints with the copper slides, allows of the resistance of the mercury rheostat being adjusted with any desired accuracy, and remaining practically constant when once correctly adjusted. The delicacy of this electrical balance is so great that a movement of 1-64th inch of one of the slides is sufficient to cause a deflection on the second galvanometer.

This balance is tried for each reading when calibrating a galvanometer.

Table III.

CALIBRATION OF GALVANOMETER G <sub>2</sub> , 22/4, 1891.			
Temperature of cell and instruments, 14.5° C.			
E.M.F. of cell, $e$ = 1.4386 volts (legal).			
Resistance of G = 430.9 ohms „ (measured).			
„ of shunt = 47.9 „ „ = $\frac{1}{9}$ G.			
Current in G = $c = \frac{10^5 e}{R + 0.1 G}$ micro-amperes.			
$\therefore R = \frac{10^5 e}{c} - 0.1 G = \frac{143,860}{c} - 43.1$ legal ohm (6)			
Column II. contains the values of R worked by this formula for the desired values of $c$ in column I., and column III. gives the observations obtained in each case; each reading being the mean of three observations.			
I. $c$ .	II. R.	III. D mm.	IV. $\frac{\Delta c}{\Delta D}$ .
5	28,729	11.3	.4348
10	14,343	22.8	.4464
20	7,150	45.2	.4386
30	4,752	68.0	.4367
40	3,553	90.9	.4425
50	2,834	113.5	.4444
60	2,355	136.0	.4474
80	1,755.2	180.7	.4514
100	1,395.5	225.0	.4444
120	1,155.7	270.0	.4395
140	984.5	315.5	.4395
160	856.0	361.0	.4348
170	803.1	384.0	

From the calibration in Table III. the volt scale was constructed on the white margin immediately below the millimetre scale by simply projecting the points of the readings in column 3 downwards by means of a set square and dividing up the sections into units. The ratios  $\frac{\text{difference of current}}{\text{difference of deflection}}$  contained in column 4 of Table III. show that the deflections are fairly proportional to the currents. Some allowance is to be made for inaccuracies in the millimetre scale apart from errors of observation.

The calibration of the ampere galvanometer,  $G_1$ , and the manufacture of the ampere scale were done in the same manner as here described.

These calibrations have been repeated several times at intervals, besides which frequent calibrations of mean readings have been taken—formulae 2 and 4 in the preceding.

It has been found that the constants of the two galvanometers have not altered beyond 1·5th per cent. during six months, except on two occasions when the levelling of the instruments had been interfered with.

For purposes like these measurements a D'Arsonval galvanometer should have a glass front to the case, so that the coil is clearly visible, and there should be means of verifying and adjusting the position of the coil in the field. The coil should be wound on a metallic (aluminium) frame, to prevent changes in its shape; and there should be a torsion head outside the cover, so that the original zero of the coil in the field can be restored without opening or interfering with the instrument.

As a slight set crept into the suspending wires of the two galvanometers used in the apparatus, and as they had no arrangement for restoring the zero, it became necessary to turn the instruments on their bases, as the position of the scales could not be altered. The effect of the shifting was hardly noticeable on one of the instruments, but it caused an alteration in the constant of the other to the extent of 1·2 per cent., and a discrepancy of ·3 per cent. in the proportionality of the scale, which necessitated re-graduating it. An alteration in the constant alone can, of

course, at once be balanced by a correction in the resistance used in series with the instrument.

New instruments fulfilling the above conditions are being designed.

*Main Ampere-Rheostat.*—This rheostat, which is marked B on the diagrams, is made of 40 platinoid strips, 6 feet long each, all joined in parallel between two copper terminal bars. The thickness of the strips is  $\cdot 03$  inch; the total width = 38 inches. The cross section is  $1\cdot 14$  square inches; the resistance is  $\cdot 0010882$  legal ohm at  $17^{\circ}$  C. The total surface of the strips, including the two terminal bars, is 5,854 square inches. When carrying 1,000 amperes 1,088.2 watts are consumed by the rheostat; *i.e.*, power is given off to the atmosphere at the rate of  $\cdot 186$  watt per square inch of surface, which has been found to cause a rise of temperature near the centre of the strips of very nearly  $20^{\circ}$  C. The rise of temperature in this rheostat has been observed with different currents, and has been found to be proportional to the square of the strength of the current.

The arrangement used for determining the resistance of B is shown in Fig. 3. It will be seen that B is placed in series

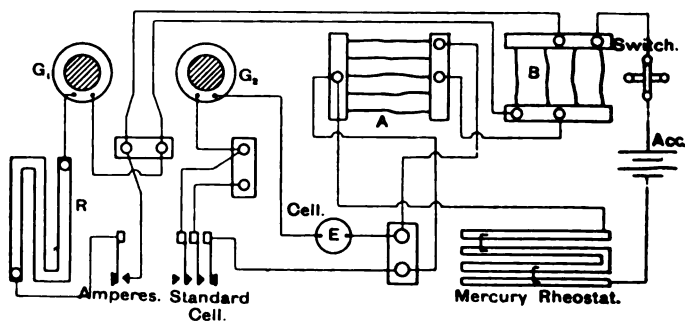


FIG. 3.

with another rheostat, A, and with the mercury rheostat previously mentioned, and that the circuit further contains a few secondary cells. The current in this circuit is adjusted by the mercury slide resistance until the fall of potential across resistance A is equal to the E.M.F. of the standard cell, which is ascertained by means of galvanometer  $G_2$ , in the manner described

previously when the calibration of the galvanometers was dealt with. The deflection is then read on the scale of galvanometer  $G_1$ , which is joined (with a resistance box in series) across the terminals of B. The resistance of rheostat A, as well as the value of the deflection on  $G_1$  and of the resistance R in series with it, being known, the necessary data for finding the resistance B are to hand. This resistance is obtained from the formula,

$$B = \frac{A}{e} c (G_1 + R) \quad \dots \quad (7)$$

where  $e$  = E.M.F. of standard cell, corrected for temperature.

$c$  = the current corresponding to the reading on  $G_1$ .

Table IV. contains the results of two determinations of B obtained at different dates.

*Table IV.*

DETERMINATIONS OF RESISTANCE OF MAIN AMPERE-RHEOSTAT.

Date.	Number of Experiments.	Resistance of B.	Remarks.
2nd April, 1891	8	·0010882 (reduced to 17° C)	Currents used through B = 29·63 and 49·36 ampere.
21st July, 1891.	3	·0010869 (reduced to (17° C)	Current through B = 49·04 ampere.

*Rheostat A.*—It is evident from the preceding part of this paper, the accuracy with which currents can be measured in the present system depends upon the knowledge of the value of the resistance of this rheostat. Great care was therefore taken in its manufacture and adjustment. It consists of five strips of platinoid, each intended to carry 10 amperes without material warming. The end of each strip is soldered to a massive gun-metal block let into a wooden frame. These blocks have holes drilled in them about  $\frac{3}{4}$  in. diameter for use as mercury cups. Two heavy strips of gun metal are let into the frame, one at each end, and there are also holes in these strips opposite the holes in the blocks. The connections are made by thick copper bridge-pieces. By this arrangement the strips can be placed

either in series or in parallel, and one or more of the strips can be placed in the circuit.

The resistance of each strip is arranged so that the fall of potential across it when a current of about 10 amperes is flowing through it is equal to the E.M.F. of the standard cell.

The absolute resistance of the strips could have varied within certain limits, but it was necessary to adjust the resistance of each to equality with a considerable degree of accuracy. As there was no means of doing this at Thames Ditton, the resistance was taken to the Ordnance Survey, Southampton, about two years ago, and the adjustment made by scraping the strips until their resistances were equal.

The resistance of the five strips in series was then taken at different temperatures, and as there are special arrangements at Southampton for keeping the temperature of the testing room constant, the temperature of the platinoïd strips could be accurately determined at the time when each resistance test was taken. The results of these tests are given in Fig. 4. The

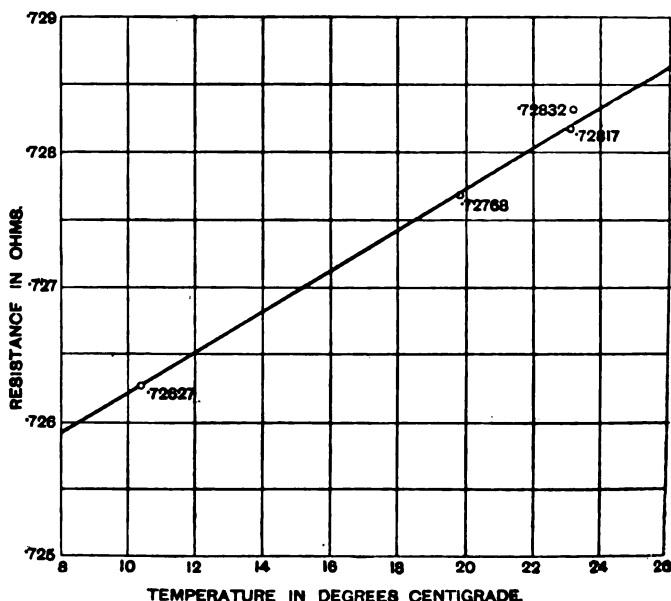


FIG. 4.

balance was made against the Ordnance Survey standard ohm (legal).

At Thames Ditton the resistance of the strips has been determined from time to time by a direct comparison with the Thames Ditton standard ohm in the manner shown in Fig. 5. It will be

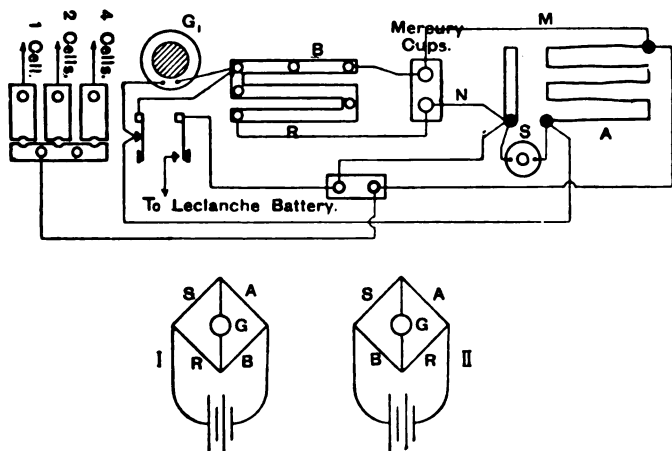


FIG. 5.

seen that the resistance and the ohm form two arms of a Wheatstone bridge arrangement, and boxes of coils the other two arms, as shown by the two small figures I. and II. The adjustable resistance,  $R$ , is first placed in one arm, and then in the other, so that the resistance,  $A$ , of the strips is found by the equation,

$$A = S \sqrt{\frac{R_2}{R_1}} \dots \dots \dots (8)$$

where

$S$  = the resistance of the standard ohm at the given temperature ;

$R_1$  and  $R_2$  the values of the adjustable coils in the first and second measurements respectively.

The only alterations required in the connections in going from the first of the two tests to the second is the changing of the two leads,  $M$  and  $N$ , in the mercury cups.

It will be observed that by this arrangement no allowance has to be made for leads, and the absolute resistance of the box of

coils is immaterial: true proportionality only is required; thus the temperature of the box of coils does not affect the result.

Fig. 5 shows that the resistance of the five platinoid strips rose from  $\cdot 7262$  ohm to  $\cdot 7280$  ohm for a rise in the temperature from  $10^{\circ}$  to  $22^{\circ}$  C.; which gives a temperature coefficient of  $\cdot 0207$  per cent. per degree centigrade. At  $14\cdot 8^{\circ}$  C. the value, according to the tests at Southampton, is  $\cdot 72693$ . The test at Thames Ditton gave  $\cdot 72659$ , leaving a discrepancy of  $\cdot 047$  per cent.

When A is used for measuring B, it is connected up in parallel, and its resistance is then 1-25th of the above.

*Surface Leakage.*—One way of quickly making a test of the state of the instruments is to send a current from the testing cells through the rheostat B, with the connections as in Fig. 3, which, when the instruments are not in use for other purposes, can be done by merely turning on a switch, and read the value of this current on the ampere galvanometer with various constants, altering the resistances in series according to Table I. When this experiment was tried after the instruments had been out of use during the Easter holidays, the galvanometer gave a higher value for the current with constant  $\cdot 5$  than with constant 1, and with constant  $\cdot 1$  still a higher value than with  $\cdot 5$ . This discrepancy, which amounted to no less than 3 per cent., was found to be due to surface leakage. It disappeared completely by a slight rubbing of the top of the resistance boxes with a piece of leather.

The considerable error mentioned was obtained with a current of about 40 amperes, producing a P.D. across the instruments of only about  $\cdot 043$  volt.

Upon the motion of the PRESIDENT, the thanks of the meeting were unanimously voted to Captain Sankey and Mr. Andersen for their interesting paper.

The PRESIDENT: I understand that Mr. Andersen would like to make a few remarks before the discussion commences.

Mr. F. V. ANDERSEN: The dimensions of the ammeter resistance, B, have been given in the paper, and it will be seen that it is rather large, being made of strips 6 feet long, with a total width of 38 inches. It has been made of that large size

The  
President.

Mr. F. V.  
Andersen.

in order to give a large range for the ammeter. If the range required were smaller, as is ordinarily the case—for ordinary daily use the required range is much smaller—then results quite as good can be obtained with a much smaller rheostat; and there is here on the table a specimen of such a rheostat (made by Messrs. Willans & Robinson) of much smaller resistance. It is made of eight platinoid strips about 18 inches long by  $4\frac{1}{2}$  inches wide by  $\cdot 05$  inch thick each; its resistance is  $\cdot 00015167$  of an ohm. It has been found that it makes a very good joint to cast the platinoid strips into the copper terminal bars. The rise of temperature, which is  $20\cdot 8^{\circ}$  C. for 1,000 amperes, is a little greater at the centre of the strips than at the ends, as was to be expected with perfect joints.

Mr. KAPP: Before entering upon the discussion of this very Mr. Kapp. interesting paper, I should be glad if Captain Sankey would more fully explain a point which was only briefly alluded to by him, namely: In Fig. 2 the platinoid resistance, A, is calibrated with five strips in series. I understand it is used with five strips in parallel. How is the change effected without introducing unknown resistances?

Professor AYRTON: The President has asked me to say some- Professor  
Ayrton. thing about this interesting paper that we have had given us by Captain Sankey and Mr. Andersen. I am extremely glad to find that both the authors are supporters of the deflectional method of measuring current and P.D., as opposed to what we may call the zero method, which has been advocated by others; that is to say, the authors support the plan of reading the deflections of two galvanometers, as contrasted with employing these instruments simply for the purpose of indicating an electric balance.

When, some years ago, I discussed with Mr. Andersen the method of testing described in the paper we have heard read to-night, I found him rather opposed to it, and I therefore consider his having been converted is a great triumph, and furnishes a strong proof that the method has real practical value. The wide range of current that can be accurately measured with a single d'Arsonval galvanometer shunting a platinoid strip has been pointed out to you this evening; but I will not dilate on

Professor  
Ayrton.

that, because, in a paper given to the Physical Society some three or four years ago on "Electrical Measurement," Professor Perry and I went very fully into the advantages obtained by using two d'Arsonval instruments, one as an ammeter and the other as a voltmeter, in the way already described to you this evening. That paper, like many others which we have read before the Physical Society, has never appeared in the Proceedings of that Society, for the simple reason that it was never written; but in this particular case a reporter appears to have been sent by the *Electrical Engineer*, since, in the numbers of that journal for March 1st and 8th, 1889, there is a *verbatim* account of the abstract which I gave on that occasion of this paper of ours on "Electrical Measurement."

I will therefore now refer rather to points where I would venture to differ from Captain Sankey and Mr. Andersen, and leave my views regarding the general value of the method to be gathered from what I said in February, 1889, as reported in the *Electrical Engineer* for March 8th of that year.

In the first place, the authors use a high-resistance d'Arsonval galvanometer when measuring amperes. I should be very glad if they would tell us why they use a galvanometer of over 400 ohms resistance when the main use of this instrument is to measure current. The d'Arsonval galvanometers which have been used for this purpose at the Central Institution for some years have only a few ohms resistance. I should also like to know why they make this galvanometer of copper when it has to be used as a voltmeter to measure the P.D. between the terminals of a platinoid sheet conveying the main current. Surely the proper thing is to make the galvanometer also of platinoid. At any rate, that is the conclusion we came to some years ago with reference to the d'Arsonval galvanometers which shunt platinoid strips at the Central Institution, and the consequence is that no temperature correction whatever has to be applied to the temperature of the room. Possibly they have some good reason for making this galvanometer of copper, which I should be glad to hear. The next question I should like to ask is whether their d'Arsonval galvanometer is so constructed as

to give a deflection directly proportional to the current with the zero at one end of the scale. Professor  
Ayrton.

When a d'Arsonval galvanometer shunting a platinoid strip is used by students, it is quite right that the sensibility should be varied by the insertion of plugs in a resistance box, and that there should be a series of constants to be employed. But I venture to think that when such a combination is intended for practical use in a workshop, and not for teaching purposes, the employment of constants is antiquated, and very likely to lead to error. Hence, when we fitted up such a combination for the Acme Works some three years ago, Professor Perry and I decided to entirely abandon the use of constants, and to make the instrument direct-reading whichever of its various sensibilities it was used with, and, further, to render it impossible for the tester to think that it was adjusted for one sensibility while it was at the moment actually adjusted for another. In fact, "we were "desirous," to quote from the *Electrical Engineer* of March 8th, 1889, "of making direct-reading scales to be used with large range "of current, and of making the arrangement such that, instead "of having to calculate what 100 divisions meant, a new scale "could be put in place of the old scale, and the current "could then be at once read off in amperes, milliamperes, deca-amperes, or deciamperes, &c., corresponding with the different "resistances put in. But the difficulty we met with was that with "the ordinary d'Arsonval galvanometer the deflections, in spite "of the very small angle through which the coil moves, are not "proportional to the current. If you start from the centre so "that the plane of the coil is along the lines of force when no "current is passing, the deflection is proportional to the current; "but if you start with the coil deflected, so that the spot of "light is at one extreme end of the scale for no current, you find, "on carefully calibrating the instrument, that you get a broken "straight line consisting of two straight lines meeting at an "angle, or, probably, strictly, meeting according to some curve "at about the spot corresponding with the plane of the coil "being parallel to the lines of force. Hence there is a difficulty in subdividing the scale uniformly, and this difficulty

Professor  
Ayrton.

“ is met with even when the plan as proposed some six years ago—that is, in using curved pole-pieces—is employed. This puzzled us for a very long time. It occurred to us that the effect might be due to the torsional wires being so placed in the ordinary d’Arsonval instruments that there is practically no twist in them when the plane of the coil is along the lines of force and the spot of light is in the middle of the scale. In order to send the spot of light to one end, the top or the bottom suspension has to be turned, and as in the ordinary d’Arsonval galvanometers the bottom suspension cannot be twisted round, the spot of light is brought to the end of the scale for zero current by turning the top suspension through twice the angle turned through by the coil. Hence, at the new position at one end of the scale, both the bottom and the top suspensions have a twist in them in opposite directions of an amount equal to the angle through which the coil has been turned from the symmetrical position. Now we thought that the calibration curve, being a broken straight line, might in some extraordinary way be due to the fact that under these circumstances, when the coil passed through the zero position, the direction of twist on the bottom suspension became reversed; but neither theory nor a number of experiments bore out this idea, which had to be abandoned, and, finally, we arrived at the conclusion that the peculiar result was due to the centre of gravity of the coil not being exactly in the axis of suspension. Hence, the coil was taken down and more accurately re-suspended by Mr. Smith, then a fourth-year student at the Central Institution. But we found, no matter how accurately he balanced the coil, the break in the calibration curve could not be entirely removed. Finally, however, after much experimenting, our assistant, Mr. H. B. Bourne, to whose dexterity of hand and fertility of resource we are much indebted, solved the problem by suspending the coil by a single torsional suspension from the top, and arranging an extremely elastic non-directive suspension to lead out the current from the coil at the bottom; in fact, by obtaining practically a unifilar torsional suspension, at the end of which the coil hangs freely, with its centre of gravity strictly in the

"axis of suspension, he solved the problem. The result is that *the calibration curve of a d'Arsonval galvanometer so constructed and drawn to such a scale that it is about a yard long, fails to show any want of deviation from perfect straightness.*"

Professor  
Ayrton.

To render it impossible for the tester to be using one sensibility while he thinks by mistake he is employing another, the following device has been adopted. On a hexagonal roller are attached six circular scales, only one of which can be seen at a time through a wide slit in the side of a long box in which the roller is placed. One of these scales is graduated in equal divisional spaces from 0 to 1 ampere, the second from 0 to 10, the third from 0 to 100 amperes, &c.; and on turning this roller the resistance in series with the d'Arsonval galvanometer is automatically varied by an amount corresponding with the change in sensibility. If, then, the tester desires to calibrate an ammeter reading, say up to 70 amperes, he turns the roller so as to show the 0 to 100 ampere roller scale; whereas, if the commercial ammeter to be calibrated has a maximum reading of 0.7 ampere, he would turn the roller so as to show the 0 to 1 ampere scale. In both cases he has the same length of roller scale to deal with, and therefore the proportional accuracy of the graduation of the commercial ammeter scale will be the same; secondly, since the resistances in series with the d'Arsonval galvanometer are automatically varied on turning the roller, there is no possibility of the 0 to 500 ampere roller scale being seen, while the maximum deflection of the d'Arsonval galvanometer is really produced by some wrong current, such as 100 or 1,000 amperes.

The discussion was adjourned until Thursday, the 26th November.

The Two Hundred and Twenty-seventh Ordinary General Meeting of the Institution was held at the Institution of Civil Engineers, 25, Great George Street, Westminster, on Thursday evening, Nov. 26th, 1891—Mr. ALEXANDER SIEMENS, Vice-President, in the Chair.

The minutes of the Ordinary General Meeting held on Nov. 12th were read and confirmed.

The names of new candidates for election into the Institution were announced and ordered to be suspended.

The following transfers were announced as having been approved by the Council :—

From the class of Associates to that of Members—

Major A. H. Bagnold, R.E.	Augustus Eden.
Walter Clark.	William Groves.
George Driver.	Capt. A. M. Stuart, R.E.
Henry Justus Eck.	W. Howard Tasker.

G. A. Zobel.

From the class of Students to that of Associates—

William Rowland.	Henry Walker.
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The CHAIRMAN : It is now necessary to appoint two scrutineers for the ballot for new members, and I would ask two gentlemen to volunteer. A Member and an Associate would be best.

Mr. Fleetwood, Member, and Mr. Richard Aylmer, Associate, were appointed scrutineers.

The CHAIRMAN : The discussion on the paper by Capt. Sankey and Mr. Andersen should now be continued, but I understand that Capt. Sankey wishes to make a few further remarks, and I therefore call upon him first.

Captain SANKEY: At the end of last meeting Mr. Kapp asked what error was involved in using resistance A in parallel, it having been measured in series. A drawing (Fig. 6) has there-

Captain Sankey.

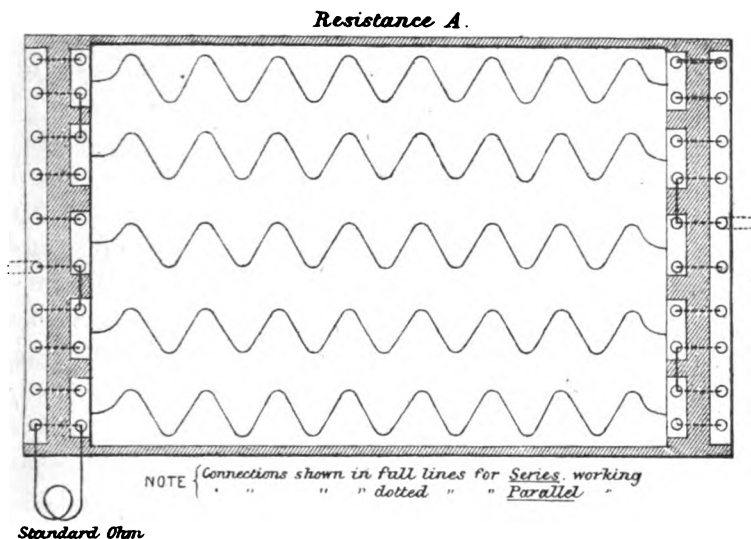


FIG. 6.

fore been prepared of this resistance, but for a description of it reference is made to page 531.

When resistance A is arranged in series for comparison with the standard ohm, the copper bridge-pieces are placed in the mercury cups as shown in full lines; there are five of these bridge-pieces. When arranged in parallel, the bridge-pieces are placed as shown in dotted lines, and there are ten of them on each side. The result of this arrangement is to practically eliminate errors due to mercury contacts, as will be seen by the following. The resistance in series is five times the resistance of one strip plus five times the resistance of a bridge-piece with its two mercury contacts, or  $= 5s + 5m$ . But when in parallel the resistance is  $\frac{1}{5}s + \frac{1}{5}m$ , which is equal to 1-25th of the resistance in series. To test the above, measurements were made three days ago of resistance A when placed in series, and it was found to be .72678 ohm at 11.4° C.; 1-25th of which is .029071. The resistance was then measured in parallel by the method

Captain  
Sankey.

shown in Fig. 5. The first measurement—Fig. 5 (i.)—gave ·029082, and the second measurement—Fig. 5 (ii.)—gave ·029075; the mean of the two measurements in parallel is ·029078, which is the same as 1-25th of the resistance in series.

At the beginning of the paper it was mentioned that the results obtained were in practical agreement with the measurements previously obtained by the Siemens dynamometers. Some figures on this point may perhaps be interesting. On the 12th May, 1891, the constant of Siemens dynamometer No. 2706 was obtained by the apparatus, and found to be 28·50 (the mean of six readings); whereas the constant given of this instrument was 28·67. The dynamometer was then sent to Messrs. Siemens for re-calibration, and was returned with a constant 28·41.

Professor  
Ayrton.

Prof. AYRTON: As time did not allow of my finishing my remarks on the last occasion, I had no opportunity of expressing my admiration of the thorough way in which Messrs. Willans & Robinson have taken in hand the fitting up of testing appliances. Many constructors of steam engines content themselves with merely turning out the manufactured article, and leave the purchaser to find out as he best can what the performance of the engine may be. But Messrs. Willans & Robinson, as is well known, pursue quite the opposite course of furnishing the would-be buyer with every means for testing the actual value of the article to be bought. It therefore became necessary for this firm to construct every engine well up to the specification, and hence this development of the testing arrangements at Thames Ditton has gone hand in hand with obtaining a wonderful degree of perfection and economy in the engines themselves.

If, then, I appear critical regarding some points in the paper read at the last meeting by Captain Sankey and Mr. Andersen, Messrs. Willans & Robinson will know that I am so from a desire to aid them in putting their arrangements for electrical testing on a par with those for engine testing, the completeness of which must have struck all visitors to Thames Ditton. And I feel sure that should I, or any other speaker in this discussion, succeed in throwing out any suggestion of moment, Messrs. Willans &

Robinson will be the first to appreciate the force of such a suggestion. Professor Ayrton.

On the last occasion, I drew attention to the method of constructing a d'Arsonval galvanometer so that the deflection of the spot of light right across a long scale, measured from a zero at one end of the scale, should be directly proportional to the current. Fig. 1 shows one of the galvanometers constructed in

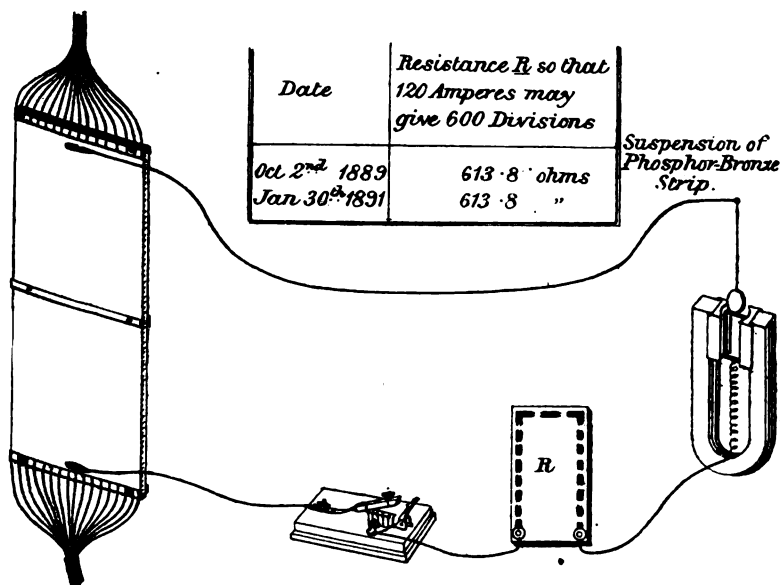


FIG. 1.

this way, and which has been in daily use in one of the laboratories of the Central Institution for some years past. The soft iron core has been omitted in this illustration merely to enable the curved soft iron pole-pieces to be better seen, but I shall have to speak later on of a d'Arsonval galvanometer which we have constructed without any iron core at all. The bent continuous line on Fig. 2 shows the exact way in which the deflection varied with the current before the coil was suspended freely at the bottom and before the curved pole-pieces were added, the divergence of the deflection from being directly proportional to the current amounting to 2.7 per cent. in the middle of the scale with this instrument, and to 3 per cent. with

Professor  
Ayrton.

some d'Arsonval galvanometers. The dotted line shows the calibration curve after these changes had been made, and it is to be noticed that this dotted line is *absolutely straight*.

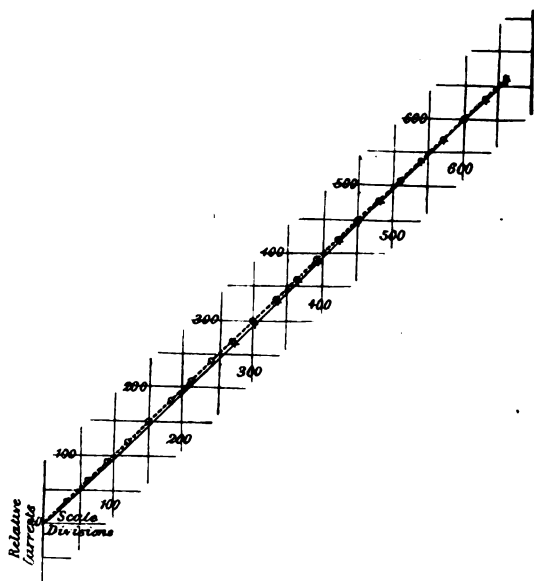


FIG. 2.

Fig. 2 also illustrates the fact that there is no necessity to use a large sheet of squared paper merely for recording such a calibration curve. Strips cut diagonally at an angle of  $45^\circ$  from a continuous roll of paper are not only more convenient to use and to keep, but, when a number of calibration curves for different instruments have to be drawn, the total amount of squared paper used up in this way is, of course, far less than if a whole rectangular sheet be devoted to each instrument.

In Fig. 1 is seen a very good form of key, constructed by Mr. H. B. Bourne when he was one of our assistants. The key has the advantage of affording a large rubbing contact, with a simple and cheap form of construction.

As regards the variation of sensibility of a d'Arsonval galvanometer with time, the following are the results obtained with two of our galvanometers, one having been periodically tested for nearly four years, the other for 15 months; the latter

instrument was also tested for four years, but having had its coil, which was originally wound with copper, re-wound with platinoid in October, 1889, the constant was, of course, entirely altered, so that the continuous record of the behaviour of this second galvanometer does not date back longer than October, 1889.

Professor  
Ayrton.

The first instrument is used as a voltmeter, and we record its constant by the actual current in amperes, which gives a deflection of 600 divisions on a certain scale, both scale and galvanometer being always kept in fixed positions. The second d'Arsonval galvanometer is the one shown in Fig. 1, and, as it is always used to measure the P.D. at the terminals of two platinoid strips soldered together in parallel, as seen in that figure—in other words, as it is always employed to measure indirectly the current passing through these strips—we record the constant of this instrument by the resistance that has to be inserted in  $R$  so that a current of 120 amperes flowing through the platinoid strips shall give a deflection of 600 divisions, the galvanometer and its scale being kept always in a fixed position.

The following are the results obtained :—

*Voltmeter d'Arsonval Galvanometer.*

Date.	Amperes required to produce a Deflection of 600 Divisions.
January, 1889	0·0001697
October, 1889	0·0001725
January, 1891	0·0001768
October, 1891	0·0001772

*Ammeter d'Arsonval Galvanometer.*

Date.	Resistance to be put in Series with the Galvanometer so that 120 amperes passing through the platinoid strips give a Deflection of 600 Divisions.
Oct. 2nd, 1889	613·8 ohms.
Jan. 30th, 1891	613·8 „

Professor  
Ayrton.

There is one great difference in the construction of these two galvanometers which may account for the sensibility of the one varying about 4 per cent. in four years, while that of the latter has not altered perceptibly in 13 months. The coil of the former is suspended as the instrument maker frequently suspends it, viz., by means of a German silver wire, while the coil of the latter we have ourselves suspended with a narrow thin strip of phosphor-bronze. Now the disadvantage of using German silver as a metal for suspensions, and the importance of employing a thin strip instead of a round wire, are hardly yet fully realised.

#### PHOSPHOR-BRONZE STRIP VERSUS GERMAN SILVER WIRE FOR SUSPENDING GALVANOMETER COILS.

German silver has not only more sub-permanent set than phosphor-bronze, as shown by two independent sets of experiments made on a number of alloys for Professor Perry and myself by Mr. Bower about 1884, and by Mr. H. B. Bourne about 1887, but it is a substance that easily undergoes chemical change in the atmosphere. An instrument, then, in which the coil is suspended with German silver will not only have a greater zero error than if the suspension be made of phosphor-bronze, but its sensibility will change more rapidly with time.

Some instrument makers employ a circular platinoid wire for suspending the coil of a d'Arsonval galvanometer. But platinoid has all the objections possessed by German silver, with the additional disadvantage, in the case of coils of low resistance, that the specific resistance of platinoid is even higher than that of German silver, so that the mere suspension introduces a resistance of 10 ohms into the galvanometer when a platinoid wire is employed.

But whatever material be used for the suspension, the following considerations will show the very great advantage of using a thin flat strip, and not a round wire, for the suspension. The formulæ from which the numbers are calculated are based on those developed by Professor Perry and myself in our paper on "A New Form of Spring for Electric and other Measuring

"Instruments,"\* and their accuracy has since been tested by Professor  
Ayrton. hundreds of experiments made by Professor Perry's students on the strength and stiffness of wires and strips of circular and rectangular cross sections. The formulæ are as follows:—

If  $N$  be the modulus of rigidity of the material used for the suspension, which in one case consists of a circular wire of diameter  $d$ , and in the other of a strip  $a$  broad and  $b$  thick, where  $a$  is more than five times  $b$ , then the couple required to give a twist of one radian per unit length will be

$$\frac{\pi N d^4}{32} \text{ for the round wire, and}$$

$$\frac{N a b^3}{3} \text{ for the strip.}$$

And the greatest shear stress on the material when a twist of one radian per unit length has been given to the suspension will be

$$\frac{1}{2} N d \text{ for the round wire, and}$$

$$N b \text{ for the strip.}$$

Now, if we take the case of two suspensions made of the same material, one a circular wire of 0.01 inch in diameter, and the other a strip 0.02802 inch broad and 0.002802 inch thick, the cross section will in each case be  $7.854 \times 10^{-5}$  square inches, so that they will have the same tensile strength and the same electric resistance for the same length and at the same temperature.

But from what is given above it follows that the couple required to give a twist of one radian per unit length will be

$$9.819 \times 10^{-10} \times N \text{ for the round wire, and}$$

$$2.054 \times 10^{-10} \times N \text{ for the strip;}$$

and the greatest shear stress, which in the case of the strip will be at the middle of the longest side of the rectangular section, will be, for a twist of one radian in unit length,

$$5 \times 10^{-3} \times N \text{ for the round wire, and}$$

$$2.802 \times 10^{-3} \times N \text{ for the strip.}$$

The surface of the suspension for one inch length will be

$$0.031416 \text{ square inch for the round wire, and}$$

$$0.06164 \text{ square inch for the strip.}$$

Professor  
Arton.

We may therefore conclude that for two d'Arsonval galvanometers in which equal lengths of these suspensions are employed respectively—

1. With coils of the same shape and volume the same current-density will produce nearly five times as large a deflection on the instrument with the strip as on the instrument with the round wire.
2. For the same deflection of the coil the greatest stress in the material of the strip will be not much more than half that in the material of the wire, therefore the liability to zero error will be much less with the strip.
3. The cooling surface per unit length of the strip will be nearly twice that of the wire.
4. The cross sections of the strip and wire have been taken equal.

Therefore, in addition to the same current-density producing five times as large a deflection when the coil is suspended with the strip as when suspended with the wire, the zero error for equal deflections will be much less with the strip. Lastly, for the same rise of temperature in the suspension the strip can carry 41 per cent. more current than the wire—a result of considerable importance when a relatively large current has to be sent through the d'Arsonval galvanometer.

Next, instead of considering a strip which has the same cross section as the round wire 0.01 inch in diameter, let us consider a strip of 0.002802 inch in thickness, and of such a breadth,  $a$ , that the same couple will produce the same twist for the same length of suspension : then

$$\frac{a \times (0.002802)^3}{3} = 9.819 \times 10^{-10};$$

$$\therefore a = 0.1339 \text{ inch};$$

and, as before, the greatest shear stress in the material of the strip will be, for one radian twist per unit length,

$$2.802 \times 10^{-3} \times N.$$

But the cross section of the strip will be now  $35.5 \times 10^{-5}$  square

inch, and the surface per inch length of the strip will be 0.2734 square inch. Professor  
Ayrton

For the two d'Arsonval galvanometers in which equal lengths of these suspensions are employed respectively we may therefore conclude that—

1. With coils of the same shape and volume the same current-density will produce equal deflections with the two instruments.
2. The maximum stress in the strip will be only a little more than half that in the wire for the same deflection.
3. The cross section of the strip will be more than four times as great as that of the wire.
4. The cooling surface per unit length will be nine times as great with the strip as with the wire.

Hence, in addition to there being much less zero error with the strip than with the wire, the resistance of the strip will be less than one quarter of that of the wire for the same length; hence for the same rise of temperature the strip will carry more than six times the current, for, in addition to having less than one quarter the resistance, it has nine times as much cooling surface as the round wire for the same length.

The scales that we fitted up at the Acme Works are seen in Fig. 3, which represents a hexagonal roller, each face of the

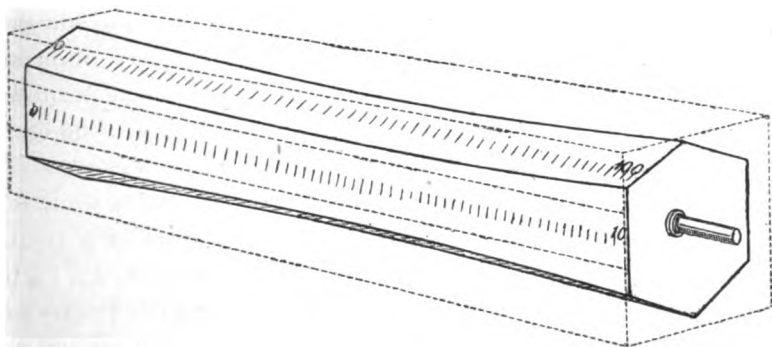


FIG. 3

hexagon being part of a circle whose radius is equal to the distance between the scale and the mirror of the instrument.

Professor  
Ayrton.

The roller is inside a wooden box, indicated by dotted lines, and only one face of the roller, and consequently only one scale, can be seen at one time through a groove in the front of the box. Attached to the end of the roller is a brass disc with six notches in it, and the roller is turned until a spring catch engages in one of these notches. The roller, therefore, is placed in one or other of six different positions, exposing one or other of the six scales, which are graduated respectively from 0 to 10 amperes, 0 to 50, 0 to 100 amperes, &c.

And, as already stated, the mere act of turning the roller from one position to the other automatically causes the resistance in series with the d'Arsonval galvanometer to be varied by a device not shown in Fig. 3. Hence, in whichever one of the six positions the roller be placed, the tester may be quite sure that the reading of the scale and the resistance in series with the galvanometer are in accord.

We have already seen that if the coil in the d'Arsonval galvanometer be suspended with a strip of phosphor-bronze, there is not much chance of the sensibility of the instrument changing. At the same time, if such a change did occur, it would be extremely inconvenient when using these direct-reading scales combined with the automatic variation of resistance which accompanies the turning of the roller, seeing that the resistance in series with the galvanometer for each of the six positions of the roller would require readjustment. Hence we provide the d'Arsonval galvanometer with a magnetic shunt, one form of which is seen in Fig. 4, which illustrates a galvanometer made for the Central Institution by Messrs. Nalder Brothers, the brass cover having been removed to enable the interior to be seen.

Should the sensibility of the instrument be found to diminish with time, the head,  $H$ , is slightly turned; this revolves a right- and left-handed screw, causing the pieces of soft iron,  $P_1$ ,  $P_2$ , to slightly recede from one another, thus increasing the proportion of the total number of lines of force produced by the permanent magnet that pass through the coil.

This brass head  $H$  is kept detached from the instrument to prevent any accidental turning of the screw and alteration of the

sensibility of the galvanometer; but when desired the head can be inserted through a hole in the brass cover and used without removing the cover. Professor Ayrton.

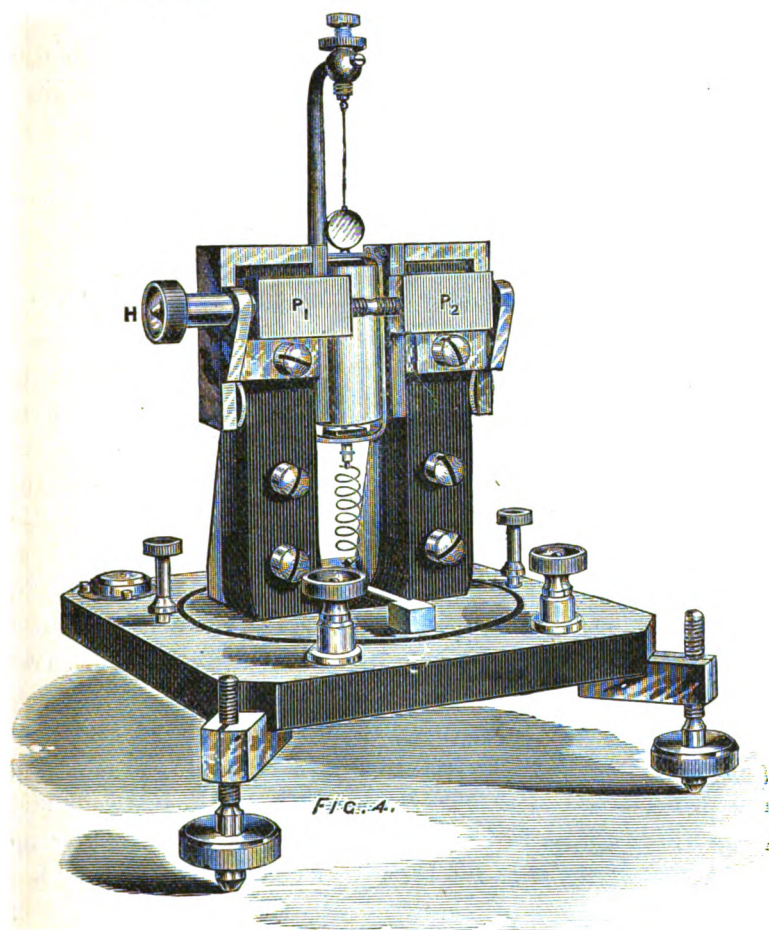


FIG. 4.—D'Arsonval Galvanometer with Magnetic Shunt.

#### BEST SHAPE FOR THE COIL OF A D'ARSONVAL GALVANOMETER.

I next come to the consideration of the shape that should be given to the coil of a d'Arsonval galvanometer. In a paper read by Mr. Mather before the Physical Society in March, 1890,\* he showed that for a given time of oscillation of a coil, and for a

\* "On the Shape of Movable Coils in Electrical Measuring Instruments."

Professor  
Ayrton.

given current-density in the coil, the best effect was obtained by winding the coil so that its cross section at right angles to the axis of rotation consisted of two circles having a common tangent at right angles to the axis of rotation. He further showed that the ordinary method of winding the coil of a d'Arsonval galvanometer, or of a Siemens dynamometer, necessitated for a given current-density either that the periodic time was twice as great as it need be, or that the deflection was only half as great as it might be. He further proved that if the coil were wound shuttle fashion the result obtained would be about three-quarters as good as if the theoretically best section had been given to the coil.

As the shuttle-wound coil has a much smaller moment of inertia than a coil of the ordinary shape containing the same volume of wire, it is necessary, for the same periodic time of vibration to be obtained, that the suspension that carries the shuttle-wound coil should introduce a less control than that which supports the ordinary shaped coil. But since, as I have pointed out, the use of a wide thin strip enables the couple that is required to produce one radian deflection per unit length to be made as small as we like without diminishing the cross section—that is, without diminishing the strength of the suspension to resist a longitudinal force, or increasing its electric resistance—it follows that the proper method of procedure is as follows :—

Having decided on the length and gauge of wire that is to be used in winding the coil of the d'Arsonval galvanometer, construct the coil of as nearly as possible the theoretically best shape as illustrated in Mr. Mather's paper. Next select a strip of phosphor-bronze of such a breadth and thickness as will possess sufficient longitudinal strength to carry the coil without risk of breaking, and as will cause the coil to have the desired periodic time of vibration : then in a given magnetic field, and for a given current, a greater deflection will be obtained with this coil than with any other wound with the same length and gauge of wire.

Fig. 5 shows a Pitkin galvanometer made with a coil of this shape, and in which there is, of course, no iron core. The magnetic field in the Pitkin d'Arsonval galvanometer is produced by a number of thin horizontal circular magnets instead of by a

few vertical ones, as in the Nalder pattern illustrated in Fig. 4; and the strength of the field in the Pitkin form shown in Fig. 5, in spite of the greater air space caused by the absence of a stationary iron core, and the magnets being smaller and lighter, is about 50 per cent. greater than in the Nalder type.

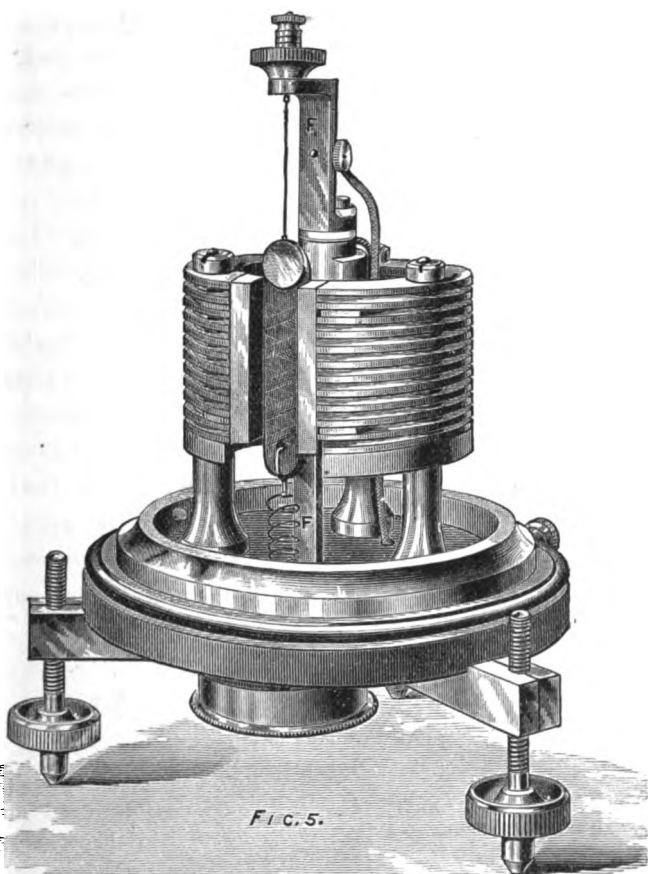


FIG. 5.—Narrow-Coil Coreless d'Arsonval Galvanometer.

The shuttle-shaped coil seen in Fig. 5 is wound with platinoid wire, and has a resistance of 13·5 ohms; the top and bottom suspensions together have a resistance of 3·5 ohms, making a total of 17 ohms for the whole instrument.

The periodic time is 2·6 seconds, and one-tenth of a milliampere

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produces a deflection of 142 scale divisions when the distance of the scale from the mirror is equal to 2,000 scale divisions. If, for the purpose of comparing the sensibility of this instrument with that of the numerous other galvanometers of the d'Arsonval and other types contained in the table attached to the paper on "Galvanometers" read by Mr. Mather, Dr. Sumpner, and myself before the Physical Society, we imagine the periodic time of the narrow-coil Pitkin d'Arsonval to be increased to 10 seconds, and the coil wound with such a gauge of copper wire that its resistance for the same volume would be 1 ohm, we find that 1 micro-ampere would produce a deflection of 28 divisions on the scale in question.

This is larger than we have obtained with any other d'Arsonval galvanometer of anything like the same size. For the next most sensitive d'Arsonval galvanometer of about this size of which we have particulars is one made by MM. Carpentier with a copper coil of 208 ohms, with which  $\frac{1}{1,040}$ th of an ampere deflects the spot of light over 4 feet and half an inch on a scale placed 4 feet 8 inches away from the mirror when the periodic time is 0.8 second. Now, assuming that this Carpentier galvanometer were wound with a copper coil of 1 ohm, that its periodic time were 10 seconds, and that the distance of the scale from the mirror were made equal to 2,000 scale divisions, 1 micro-ampere would produce a deflection of 26 scale divisions.

At first sight it would appear, then, that while the narrow-coil Pitkin galvanometer was more sensitive than that made by MM. Carpentier, the difference in sensibility was but slight; but it has to be borne in mind, as Mr. Mather has pointed out to me, that *the moving coil of the Carpentier galvanometer has been constructed so as to be as light as possible; and no attempt has been made to render this galvanometer dead-beat on open circuit*, whereas with the narrow-coil Pitkin form illustrated in Fig. 5 as much as six-sevenths of the weight of the coiled bobbin is due to the copper bobbin on which the coil has been wound, this bobbin having been made of this relatively massive character in order to obtain considerable magnetic friction and dead-beatness.

Hence, were this heavy copper bobbin replaced by a light

framework, as in the d'Arsonval galvanometers made by MM. Professor  
Ayrton Carpentier, the narrow-coil Pitkin form would be many times as sensitive as the Carpentier pattern for the same periodic time and resistance of the coil.

Apart from the special shape of the coil shown in Fig. 5, this method of constructing a d'Arsonval galvanometer has the great convenience that the coil, with its top and bottom supports, can be at once bodily withdrawn, by sliding the framework *F F* out of the tube that supports it in position.

Mr. Crompton has asked me a question regarding the use of electro-magnets in place of permanent magnets in a d'Arsonval galvanometer. That point was referred to in our paper on "Galvanometers," when considering this question of narrow coils, for we said: "The sensitiveness could be further increased by employing electro-magnets instead of permanent ones to produce the deflecting field, the current flowing round the electro-magnet being kept constant by means of an auxiliary reflecting 'set-up ammeter,' producing an image on the same scale as is used for the electro-magnetic d'Arsonval galvanometer. With a 'set-up ammeter' the suspended system is supported by means of an almost torsionless phosphor-bronze strip, requiring many twists to be given to it to bring the spot of light on to the scale when the current that is to be kept constant is flowing through it. A motion, then, of the spot of light over 100 divisions of the scale corresponds, perhaps, to a variation of only  $\frac{1}{20}$  in this current; and consequently, by means of such an instrument and a suitable adjustable resistance, a current can be kept constant to a very small fraction per cent."

The coils of the two d'Arsonval galvanometers at Messrs. Willans's works are each wound with copper wire, and have a resistance of 429 and 435 ohms respectively, although one of them need only measure a maximum P.D. of about 1 volt, seeing that the maximum P.D. it is required to measure is that produced between the terminals of a platinoid sheet of about 1-1,000th of an ohm in resistance when conveying the maximum current of 1,100 amperes. This, I venture to think, is a distinct mistake, seeing that the employment of copper

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in this galvanometer requires a correction to be applied in the measurement of current for every variation of temperature of the room, apart altogether from the correction necessitated with the Thames Ditton apparatus for the change of resistance of the platinoid sheet produced by the passage of a large current through this sheet; whereas, if the coil of the galvanometer were wound with platinoid wire, the necessity of making a correction for variation in the temperature of the room would be altogether avoided.

When calculating the change in sensibility produced in a d'Arsonval galvanometer by altering the thickness, or the material, of the wire with which the coil is wound, we must not forget that the suspension itself is frequently made of platinoid wire by well-known instrument makers, and that under these circumstances the resistance of the suspension itself is 10 ohms. Assuming this to be the resistance of the suspension in the galvanometers at Thames Ditton, it is easy to show that if no alteration whatever were made on the platinoid sheet used to convey the main current, but if the 429-ohm copper coil of the d'Arsonval galvanometer, used to measure the P.D. at the terminals of this sheet, were replaced by a 10-ohm platinoid coil of the same size (and which would be, of course, cheaper to make), the deflection for a given current passing through the sheet would be increased by 25 per cent., and the temperature coefficient reduced to 1-18th of its value for the copper coil.

It has been objected that such a galvanometer could not be used to calibrate the 435-ohm copper-coil voltmeter d'Arsonval galvanometer by the method described in the authors' paper. Perhaps, if an accuracy of 0.011 per cent.—which is what the authors profess to obtain in this particular calibration—were necessary, the 10-ohm platinoid-coil d'Arsonval galvanometer, the use of which I recommend, would not do. But what is the use of this high degree of accuracy in this particular calibration, seeing that in the commencement of their paper the authors give, among “the more important conditions imposed in “designing the apparatus,” that “(4) The errors to which the “measurements are liable should not exceed one-fifth per cent.”?

*Further, if the d'Arsonval galvanometer which the authors use as a voltmeter were calibrated by the exact method they describe in their paper with a Clark's cell, and if the proposed 10-ohm platinoid-coil d'Arsonval galvanometer were used as the auxiliary galvanometer in this calibration, it is easy to show that, with exactly the same degree of accuracy of reading the scale of this instrument as that assumed by them, the former instrument could be calibrated with an accuracy of 0.14 per cent.—that is, with a much greater accuracy than the one-fifth per cent.*

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It has also been objected that if the resistance of the d'Arsonval galvanometer which shunts the platinoid sheet were reduced from 429 to 10 ohms, and if, further, the material used in winding the coil were changed from copper to platinoid, the rate of production of heat in the galvanometer coil would be much increased for the same current passing through the sheet.

This is perfectly true; but calculation shows that, even with this increased rate of production of heat in the 10-ohm platinoid coil, the temperature of the coil would not be raised by 1° C. in several hours even if the largest current, 1,100 amperes, were kept flowing continuously through the sheet, and even if we assumed that there was no loss of heat whatever from the coil either by radiation, convection, or conduction. And similarly it can be shown that even if the resistance coils put in series with the d'Arsonval ammeter were wound with the ordinary thickness of wire employed for such coils, the rise of temperature would be insignificant, even if the 1,100 amperes were kept flowing for many hours. Hence it follows that this objection to the use of a low-resistance platinoid-coil d'Arsonval galvanometer has no more weight than the previous one.

And if, while replacing the 429-ohm copper coil with the 10-ohm platinoid one, the suspension of platinoid wire be also replaced by one made of our thin phosphor-bronze strip of such dimensions that, while it offers the same mechanical resistance to being twisted, it offers a much less resistance to the current, the sensibility of the 10-ohm platinoid d'Arsonval galvanometer will be much greater than that which I have just given.

Next, in the actual use of their instruments do Captain Sankey

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and Mr. Andersen really obtain the accuracy of one-fifth per cent. which they take as the basis in the fitting up of the apparatus which forms the subject of their paper?

When a constant between 0.05 and 1 volt per division with the voltmeter is desired, they employ "1 potentiometer rheostat" of 250 ohms resistance. The P.D. to be measured is maintained between the terminals of this 250 ohms while the d'Arsonval galvanometer, plus a resistance, is used as a shunt to 1-100th of the rheostat that is used to shunt the 2.5 ohms. In Table II. of the authors' paper it is stated that when, with these connections, the resistances of 60 and 9,469 are respectively added to that of the galvanometer, the constants are 0.05 and 1 volt per division; or, since the resistance of the d'Arsonval galvanometer employed is given as 435 ohms, this is equivalent to saying that when 2.5 ohms of the potentiometer are shunted with 495 and 9,904 ohms respectively, one division deflection means a P.D. at the terminals of the potentiometer of 0.05 and 1 volt respectively.

But this is obviously incorrect in the light of an accuracy of 1-5th per cent. being sought after. For, if the sensibility of the galvanometer be such that 1 volt gives a deflection of one division when there is a resistance of 9,469 ohms in series with the galvanometer, the P.D. that will produce a deflection of one division when 60 ohms is in series must be

$$\frac{1}{2,000} \times \frac{247.5 + \frac{2.5 \times 495}{2.5 + 495}}{\frac{2.5 \times 495}{2.5 + 495}},$$

or 0.0502, and not 0.05 volt. So that this neglect of the diminution of the resistance produced by using the galvanometer as a shunt makes an error of 0.4 per cent., apart from all errors due to temperature, errors of graduation of the scale, difficulties of reading, &c.

Again, it is said in the paper that "unit deflection on the volt scale is produced by a current through the galvanometer of "1 micro-ampere;" hence it follows that when 60 ohms is in series with their d'Arsonval voltmeter of resistance 435 ohms the constant will be 0.000495. But in Table II. we are told that the

constant is 0.0005, which differs from the true constant by 1 per cent. Professor  
Aytton.

The authors further state that "for ordinary work the readings on the scale are amply accurate, as they can be depended upon to be within 0.5 per cent. of error;" but this degree of accuracy they are only able to obtain by graduating the scale by hand. We, on the contrary, find that, if the coil of a d'Arsonval galvanometer be properly suspended, and properly shaped curved pole-pieces be employed, the readings on the scale can be trusted to 0.2 per cent.; and when, in addition, we use the calibration curve with our galvanometers, the current can be read to an accuracy of 0.1 per cent.

#### METHOD OF CALIBRATING A D'ARSONVAL GALVANOMETER ARRANGED FOR MEASURING LARGE CURRENTS.

The method employed by the authors for calibrating their current-measuring arrangement consists, first, in determining the current in amperes that will produce a given deflection of the D'Arsonval galvanometer; secondly, in measuring the resistance of the platinoid sheet which is to convey the main current.

To enable the resistance to be easily measured, this sheet is made of five strips, which, by a suitable arrangement of mercury cups, can either be joined up all in series when the resistance is measured, or all in parallel when the main current passes through them. And it is assumed that the resistance of the five strips in parallel is 1-25th of the resistance when they are in series.

But in view of the well-known variation of the resistance of mercury cups with the cleanness of the mercury, and the time that has elapsed since the copper bridge-pieces were last amalgamated, it seems to me a rash assumption to make that the resistance of the five strips in parallel is exactly 1-25th of the resistance in series, especially when the whole accuracy of the subsequent measurement of the current depends on knowing the exact resistance of the platinoid sheet when the current to be measured is passing through it. In fact, this resistance is a vital factor in the current measurement.

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Recently, three of my students—Messrs. Drysdale, Poore, and Powell—have been investigating this matter, and they find that a resistance embracing a copper bridge-piece dipping into two mercury cups which have not been made in any way specially dirty, can be reduced from 0.002660 ohm to 0.000581 ohm simply by replacing the mercury that had been standing some time with fresh mercury, and by reamalgamating the ends of the copper bridge-piece.

Indeed, it is on this account that in the use of standard metre bridges all the mercury cups are refilled and all the copper bridge-pieces reamalgamated every day in some factories—a precaution I remember Dr. A. Muirhead always took when I was working with him in the works of Messrs. Latimer Clark & Muirhead some years ago.

Captain Sankey has to-night told us that actual tests made since the last meeting of this Society on the resistance of the platinoid strips at Thames Ditton have shown that the resistance of the strips in parallel is really 1.25th of their resistance when in series. But if the only way of ascertaining this fact is by an actual measurement of their resistance in parallel, why put the strips in series at all to measure their resistance, and why use any mercury cups and copper bridge-pieces?

Indeed, I would go still further, and say that the best way of measuring a small resistance for a big current passing through it is to send a large known current through the resistance and measure the P.D. at its terminals. But if this method be followed there is no occasion to measure the resistance of the platinoid sheet at all, since all we want to know is what current is passing through the strip when any particular deflection is produced on the d'Arsonval galvanometer connected with its terminals.

I therefore prefer the following method of using and calibrating a d'Arsonval galvanometer used as an ammeter:—Solder a wire to each end of the sheet, as seen in Fig. 1; insert some convenient resistance in the galvanometer circuit, the ends of which are attached to the two wires soldered to the ends of the sheet; then observe the deflection of the galvanometer when a

known large current is sent through the sheet. Use no mercury cups, bridge-pieces, screws, nor anything of a variable character in the platinoid sheet; and, further, it may be well to spread out the wires conveying the main current and solder them to different parts of the edge of the sheet, as seen in Fig. 1, so as to cause the lines of flow of the current in the sheet to be as nearly as possible the same for all currents.

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To measure this current we employ a Thomson balance, and this, I venture to think, would be quite an accurate enough standard for any commercial purpose. Captain Sankey and Mr. Andersen may say the buyer of a dynamo at Thames Ditton would not be content to take for granted the accuracy of the graduations of a Thomson balance; but as it is he takes for granted the accuracy of the Clark's cell, the accuracy of the resistance coils, and—what is more surprising still—the buyer takes it for granted that he knows a resistance which is only about the one-thousandth of an ohm to something like the one-five-hundred-thousandth of an ohm, since that is what is necessary if “the errors to which the measurements are liable should not exceed one-fifth per cent.” And apparently the buyer has had such a belief in the goodness of the steam engines manufactured by Messrs. Willans & Robinson that, on seeing the resistance of five strips of platinoid measured when in series, he has assumed, in spite of the mercury cups, that he could predict the exact resistance of these strips when put in parallel to one-five-hundred-thousandth of an ohm.

Surely it would not require much more faith to believe in the accuracy of a Thomson balance!

We at the Central Institution go a step further, and periodically calibrate our balance by the copper-deposit method, and I am happy to say that we are beginning to get successive determinations of the constant thus obtained to agree to 0.06 per cent. But this copper-deposit check would be quite unnecessary at a works like Thames Ditton.

As it appeared to me that not merely was the resistance of the galvanometer used at Thames Ditton much too high, but also the size of the platinoid sheet much too large, I asked Mr.

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Mather to consider *ab initio* the proper arrangements to employ if it be desired to measure currents up to 1,100 amperes with an accuracy of one-fifth per cent. by means of a d'Arsonval galvanometer shunting a platinoid sheet. And in giving you the result of Mr. Mather's calculations, I desire to take this opportunity of expressing my indebtedness to him for the extremely painstaking and highly suggestive manner in which for some years past he has grappled with every problem I have suggested to him.

The first point to settle is the lowest as well as the highest current that has to be measured to one-fifth per cent. According to No. 5 of the "more important conditions" in Captain Sankey and Mr. Andersen's paper—" (5) The range such as to allow of "measurements being made of current from 1-40th of an ampere "to 1,100 amperes." A little consideration, however, will show that the authors have not with the apparatus they have arranged fulfilled the conditions they started by imposing on themselves. For if we assume that the scale they employ is 500 mm. long, and that readings can be taken accurately to one-fifth of a millimetre, then it is possible to measure a current of 5 amperes to one-fifth per cent. But to carry out the condition stated in the commencement of the authors' paper of measuring a current of 1-40th of an ampere to one-fifth per cent. would necessitate, with the apparatus they employ, that the deflection should be read accurately to the one-thousandth of a millimetre.

In deciding, therefore, as to the best form to give our apparatus, we will take the range actually obtained with the Thames Ditton apparatus—viz., to measure from 5 amperes to 1,100 amperes to one-fifth per cent.—and not the imaginary range of 1-40th to 1,100 amperes with an accuracy of one-fifth per cent., as stated in the authors' paper.

Under these circumstances, Mr. Mather concludes that two square sheets of platinoid should be used—one of 5 inches by 5 inches, to be employed in measuring currents from 5 to 110 amperes, and one of 15 inches by 15 inches, to be employed in measuring currents from 50 to 1,100 amperes—the two ranges well lapping over one another for convenience of comparison; and,

further, he recommends that the 429-ohm copper coil should be replaced by a 10-ohm platinoid coil.

Professor  
Ayrton.

The following table shows the particulars of the plan at present employed at Thames Ditton, and the one we should propose for a similar purpose. The advantages to be gained by the change are—

1. No temperature coefficients whatever would require to be used, either for changes of temperature of the room or for the heating of the platinoid sheet even by the passage of the largest current—1,100 amperes; whereas with the present arrangement two independent corrections are necessary—one for the temperature of the room, and one for each current passing through the sheet.
2. The maximum resistance to be inserted in series with the galvanometer when the largest current—1,100 amperes—was being measured would be reduced from 17,700 to 60 ohms.
3. The length of the platinoid sheet would be reduced from 6 feet to 15 inches, and the breadth from 38 inches to 15 inches.
4. The total watts expended on the platinoid sheet when the maximum current was being measured would be reduced from 1,300 to 40 watts.

This last change (No. 4) would be of little importance in the testing of dynamos where all the power given off is converted into heat, but would be of the highest importance when it is desired to measure the current without wasting power, as, for example, when testing the power and efficiency of a coupled dynamo and motor, &c.

*Details of Existing and Suggested Arrangements for Testing Current at Thames Ditton.*

Arrangements.	PLATINOID SHEET.					Range of Current in Amperes, measured with 1th per cent. Accuracy.	D'ARSONVAL GALVANOMETER.		Maximum Resistance in Ohms to be added in Series with the Galvanometer.	Watts expended in Sheet when Measuring the Maximum Current.
	Length.	Breadth.	Thickness.	Total Surface in Square Inches.	Resistance in Ohms.		Material of Coil.	Resistance in Ohms of Galvanometer.		
Existing	72"	38"	0.03"	5.854	0.001088	5 to 1,100	Copper ...	429	17,700	1,300
Suggested	15"	15"	0.40"	460	0.000033	50 to 1,100	Platinoid	20	60	40
	5"	5"	0.04"	50	0.00033	5 to 110			60	4

Mr. R. E. CROMPTON: I regret that the object of this paper appears to have been misunderstood and somewhat undervalued. I think we English electrical engineers who have established a particular type of electrical generating machinery for central stations are vitally interested in the accuracy of the electrical measurements carried out at Messrs. Willans's works. It is well known that most of the large English central stations are worked by high-speed steam dynamos, the driving power in most cases being Willans's engines. So many makers of high-class dynamos have sent their engines to Messrs. Willans's works to be fitted to his engines, that his works have become a common ground on which dynamo makers meet, and can judge there of the comparative merit of their dynamos from the very impartial and accurate tests that Messrs. Willans have now been carrying on for some years. I consider that Mr. Willans's famous paper read before the Civil Engineers established the fact that his methods of measuring the indicated horse-power and steam and water consumption were the most perfect and accurate that had ever been adopted; and I think it is of great importance that we electrical engineers should ourselves be satisfied that the electrical portion of the tests, on which the efficiency of our machines depends, should be absolutely above suspicion. From check tests that I have made myself, and from the check tests that I have heard have been made by many eminent firms of dynamo manufacturers, I think

it is beyond dispute that Messrs. Willans have obtained a sufficient degree of accuracy in their electric measurements; but this paper of Captain Sankey's gives us full opportunity of satisfying ourselves on this point. Further than this, I consider a purely electrical paper of this class a most fitting one to commence the year's discussions. The great bulk of our audience, consisting, as it does, of young engineers who are carrying out the practical electrical work of this country, are every day using apparatus of this class, and, consequently, are very capable of discussing this paper in a thoroughly practical manner. They know how important and valuable it is to the profession that the various special appliances and dodges which go to make electrical testing accurate and successful should be made public.

Turning to my own private criticism of the paper, I have had considerable experience in making accurate electrical measurements. Not only have I adopted for some years past at our works the potentiometer methods of measurements introduced by Dr. Fleming, but I have myself worked out and perfected various arrangements for calibrating instruments for public laboratories, or other institutions where great accuracy is desired. In this connection, I would point out that the Board of Trade blue-book, giving the report of the Standardising Committee, states that accuracies up to 1 part in 10,000 are easily obtainable in the comparison of electrical resistances, and I know that there is a common idea in the profession that accuracies far exceeding this could be obtained in regular practice. I must say that I find it very difficult, if not impossible, to repeat from day to day measurements to the above degree of accuracy. I have purchased apparatus from the makers of the highest repute, and I find that resistance boxes which agree in themselves, or in which the ratios agree to the above degree, are extremely rare, and are seldom met with. I must point out that the surface leakage over the ebonite covers of resistance boxes, to which attention has been called by Captain Sankey, plays a much more important part than has hitherto been suspected to be the case. The atmosphere of London, particularly at this time of the year, is such that ebonite soon gets covered with a sticky conducting

Mr.  
Crompton.

Mr.  
Crompton.

film, which can introduce errors when high resistances are being measured to the extent of fully 1 per cent.

Turning now to the special use of the d'Arsonval reflecting galvanometer by Captain Sankey, I do not think it is so trustworthy as the zero or null method. I must confess that I should feel greater confidence in measurements taken by the latter method, as no disturbance or interference with the instruments is likely to cause unsuspected errors. With the null method we adopt it is very difficult for the apparatus to have been interfered with in such a manner as to introduce errors into the measurements without the experimenter being at once aware of it. So long as the Clark cells used agree among themselves, and the slide wire of the potentiometer is in fair order, it is almost impossible to carry out measurements which will fail to be accurate to the extent of a quarter of a per cent.

I am glad that attention has been called by Professor Ayrton to the d'Arsonval galvanometer. I want to ask why those making these instruments have confined themselves to the use of the French form originally introduced into this country by Carpentier, and, moreover, have always confined themselves to the use of permanent magnets. In these days, when it is so easy to obtain current to excite electro-magnets, I think that an electro-magnetic galvanometer, which would provide a much more powerful field than is possible with a permanent form of magnet, would be in many ways preferable, especially as a very delicate instrument to use with the null method.

Mr.  
Swinburne.

Mr. SWINBURNE: The system of measurement brought before us has, of course, been familiar to electrical engineers for several years, but a full description of a set of carefully worked out details is of great value. What has been arranged at Messrs. Willans's works is really calibrating gear, and such dispositions of apparatus are usually employed by instrument makers.

It may be objected that a commercial voltmeter or ampere-meter is not accurate. Without admitting this, we may assume it. All that is necessary is, then, to calibrate the instruments. Messrs. Sankey and Andersen hold that a commercial voltmeter is inaccurate, and then calibrate a reflecting galvanometer. I do

not regard a reflecting galvanometer as an engineering instrument. As an electrician I like the arrangement, as it gives a large range and fair accuracy; as an engineer I object to spot-watching. Mr.  
Swinburne.

As to accuracy of commercial instruments, there are several grades in the market. A cheap voltmeter may not be good enough for very accurate work, but may still be very excellent as a cheap voltmeter. A standard voltmeter is quite as accurate as a newly calibrated reflecting galvanometer. A letter balance is a very excellent piece of apparatus, but it is no reproach to it that it is not good enough for chemical analysis.

The foundation of all the systems of testing is the standard cell. I would like to call attention to a few points with regard to it. Some people will tell you that the Clark cell is of no use whatever, and that no two people can make them alike; others will tell you that they may always be made with certainty within one or two in a million. Now neither of these is the case. You can very easily, if you have the same chemicals, from the same place, containing the same impurities, put them together and get cells that will agree. Suppose, instead of doing that, you start with chemicals of known purity, and make them according to the instructions commonly in vogue: you will get two cells that will agree with each other within a very small percentage indeed. But it does not follow that they are right. It so happens that as soon as you use saturated zinc sulphate the temperature coefficient varies. It is therefore most important that the question of the variation of temperature coefficient in cells should be remembered. It is quite possible for measurements agreeing with standard cells which agree among themselves, and with other standard cells, made by other people, to be a half per cent. wrong.

There seems to be an impression arising that you cannot use resistances in series with an instrument because they will leak either at the surface or elsewhere. The ordinary double-wound resistances are likely to be worst. To see how far leakage could be avoided, I asked Mr. Bourne, our chief assistant, to make up a double coil, leaving the end open, and see what

Mr.  
Swinburne.

happened to it. He, of course, used the best insulation he could for the purpose. Three layers of small double silk-covered wire were wound together. The insulation resistance between them was over 600 megohms. Then he tested it for breakdown, and it ran up to 950 volts before giving way. With the least care no error from leakage or from bad insulation should occur in resistances. The series resistance ought to be absolutely trustworthy. I would like to ask those who have used platinoid much as to the permanence of the alloy. Platinoid is generally supposed to be a sort of German silver containing tungsten, and a good many of the peculiar qualities are supposed to be due to the presence of that element. Some time ago I carefully analysed a number of samples, and found no trace of tungsten. I believe it is German silver with a very large percentage of nickel, and many people have found that under certain circumstances it is apt to disintegrate. I think it is most important to know when we can depend upon platinoid, and when we cannot. Referring to measuring very low resistances, there is no difficulty in the matter. Professor Ayrton proposes to use a standard current: that is not nearly so convenient as stepping down with standard resistances. Standard currents cannot be reproduced with anything like the accuracy of standard resistances.

Mr. Crompton referred to the Board of Trade, saying we ought all to measure within 1 in 10,000. I am not sure there is not some confusion about that. In measuring electrically it does not matter whether you consider difference of potential, or current, or resistance. You are always measuring either difference of potential or current, and comparisons can be made within one in a million. What we want is a standard. There is no difficulty in comparing two standard cells within one or two in a million. It is quite a different matter to ascertain the electro-motive force of your cells.

I can only add that we ought to congratulate Capt. Sankey and Mr. Andersen on this paper; and I rather object to some expressions of Professor Ayrton, when he said something or other would be accurate enough for the workshop, but would not be accurate enough for the Central Institution. Nearly all accurate measure-

ments are made in the workshop, and not in the laboratory. The most accurate measurements made commonly are the measurements of weight, and they are carried out with the greatest accuracy in commercial analysis; the next most accurate are measurements of lengths and dimensions, and those are made with the Whitworth machines in trade workshops, not in laboratories.

Mr. EVERSLED : As we are discussing instruments which give an unusual degree of accuracy, at least for commercial work (as close as one part in a thousand), it seems worth while to enter into the nature of the errors that may be inherent in any particular instruments. I am rather surprised that Capt. Sankey and Mr. Andersen have said nothing on the subject of the temperature coefficients of their instruments, except in as far as they referred in one place to the temperature coefficient of platinoid wire which, I believe, they measured. Well, permanent magnets have temperature coefficients; springs have temperature coefficients; and so on; and all these small things ought to be investigated before an instrument which appears to be so excellent as the d'Arsonval galvanometer can really be recommended for workshop practice. We ought to know all about the instrument. I have been using for many years as a standard instrument a kind of tangent galvanometer with a controlling field due to a bar permanent magnet. Some ten years ago, if anyone had been asked if there was anything permanent in this world, he would have said he did not know, but certainly not permanent magnets; but we, most of us, now consider that we can trust permanent magnets if left alone. At all events, this particular permanent magnet gives no trouble whatever; it varied a little less than 1 per cent. during two years. It has, however, varied somewhat more rapidly since its removal to Messrs. Goolden's present works. With respect to the temperature coefficient of the magnet, I determined that at one time roughly by leaving the magnet in its place on the instrument, surrounding the whole thing with ice, and, keeping a constant current passing through the instrument, noticed the increase in deflection due to the decrease in the temperature of the magnet. The result showed a coefficient of about .01 per cent. per degree centigrade, which

Mr.  
Evershed.

for commercial purposes might probably be neglected. On the other hand, the magnets used at Kew and Dublin for observations of H and so on, have temperature coefficients of the order  $\cdot 03$  per cent.—that is to say, distinctly comparable with the coefficients of platinoid. Some time ago I was horrified to find that Professor Ewing, in the course of his elaborate researches, found a temperature coefficient for hard steel something of the order  $\cdot 3$  per cent.! Thinking there must be some mistake—either the steel was not of an ordinary quality, or there were some special circumstances to make so large a temperature coefficient—I wrote to Professor Ewing, and he agreed with me that the result looked a little extraordinary, and as he was not at the time actually engaged in determining the value of the coefficient, there is very possibly some slip in his published results.

Mr. Vignoles determined the temperature coefficients of two hard magnets made of Jowitt steel, the result in both cases being a coefficient of about  $\cdot 01$  per cent. Probably this value is not far from the truth for all thoroughly hard magnets, but it seems desirable to investigate the matter a little further. Unfortunately we manufacturers are chiefly engaged in trying to make a living, and have no time for making those elaborate experiments carried out in technical colleges.

The only other point I should like to draw attention to is a new alloy which is being used in Germany in place of platinoid, and which has a negative temperature coefficient. The resistance falls very slightly as the temperature rises. As I believe I was the first person in England to test this alloy, I may mention some results I got. The alloy comes, like most chemical ingenuities, from Germany. The first specimen we got was large in diameter, and so short that we could not really make an accurate experiment with it. We measured its resistance cold and heated it by current, and then found the resistance had fallen. I sent that piece of wire to be drawn down and covered with silk, and when it came back again we found its temperature coefficient had become positive, like all other wires. I then found that the makers were aware of that effect; they said, however, that they would undertake to draw

it down to any size I liked without spoiling it. It required drawing in some special vapour. They sent me some specimens drawn down to 2 mils diameter, and we find a negative temperature coefficient for this wire of  $\cdot 01$  per cent. This coefficient can only be relied on between about  $10^{\circ}$  and  $50^{\circ}$  C., as the alloy seems to change its properties permanently if heated to about  $160^{\circ}$  C. I introduce this alloy to you because it may be extremely useful for certain purposes; but it must never be heated beyond  $100^{\circ}$  C., although with ordinary variations of 10 or 20 degrees it is perfectly reliable, and seems an admirable metal for resistances or voltmeter coils, and so on.

Professor PERRY: What alloy is it?

Mr. EVERSHED: "Manganine" is the name, and I do not think it is known how it is made, although its composition has been published. There is one species of d'Arsonval galvanometer which does not hail from France, nor is it an English copy of French instruments. It is made in America. We always assume that Americans cannot make instruments, and that may be true generally; but there is certainly one American who can, and that is Mr. Weston. His voltmeters are only d'Arsonval galvanometers in which the current is led in by two phosphor-bronze hair springs, which act also as the controlling force. Instead of the clumsy mirror and scale, a light aluminium index is fixed to the coil, and reads on a scale like an ordinary commercial instrument. The instrument I tested could be read to a quarter-volt quite easily over the whole scale, ranging from 0 to 150 volts, and I think that is quite comparable with the accuracy Captain Sankey mentions for readings with mirror and scale with d'Arsonval galvanometers in ordinary practice. Not only is the scale of the Weston instrument accurate, but the volts, too, are accurate. Mr. Weston calibrated this voltmeter in February last, and I tested it three weeks ago and found his volts agree with ours to  $\cdot 05$  per cent. I think it is due to Mr. Weston to bring that to your notice.

Mr. A. P. TROTTER: We ought to bear in mind in discussing this valuable paper that there are three classes of measurement to consider. The one which we hear most about is the laboratory

Mr.  
Evershed.

Mr.  
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Mr.  
Trotter.

class of measurements ; the one under discussion is the workshop class ; and between the two is the instrument maker's class of measurements. I would remind the meeting that the first name this Society bore was, the Society of Telegraph Engineers. It bore that name in a day when resistance was measured in miles of telegraph wire, and electro-motive force in Daniell cells. Then the title was changed to "The Society of Telegraph-Engineers and Electricians," and laboratory instruments and methods came into use. Now we are Electrical Engineers, and it seems that the time is come when electrical engineering measurement should have a proper place. Some years ago those studying mechanical engineering found that the science of thermodynamics had got so involved and complicated that many gave it up in disgust. It was not until the historical paper of Mr. Willans, which treated of workshop thermo-dynamics, and not laboratory science, that some of us ventured to look again into the theory of the steam engine. One rather hoped that Messrs. Willans & Robinson would have shown us also how laboratory measurements should be converted into electrical engineering measurements. At a first glance, the system described appears to be a laboratory method ; and it is a laboratory method without, perhaps, the great accuracy which laboratory workers hope to get. But Mr. Swinburne has already touched upon the point of accuracy for different purposes, and although the laboratory exactness is not desired here, yet the engineering exactness of measurement is of a very high order ; and, to follow Mr. Swinburne's idea a little further, what higher accuracy is there in linear measurement than what an engineer calls "a good fit" ? Whitworth's measuring machine certainly shows an absolute measurement of one or two millionths of an inch, and an engineer's "good fit" is of the same order, but it is relative, and not necessarily absolute. When we want to produce a perfect inch gauge, we know not one fitter in two hundred could do it, and he would spend about a week upon it. To make a good fit may be compared with a null method ; to measure absolutely with a Whitworth machine may be compared with the scale readings advocated by the authors.

But the d'Arsonval galvanometer—I have not much experience in the use of it—does not seem to be exactly an instrument which has come to stay for engineering purposes. The quartz fibre has revolutionised the Thomson galvanometer, with which we are familiar, and “spot-watching” is a thing to which the engineer may have to come, but one to which he will have to bring himself to with considerable dislike. It is to be hoped that the time shortly will come when we can have a needle-reading instrument—a so-called commercial instrument—to read to one in a thousand. And why we want to go further than one in a thousand for workshop purposes I fail at present to see. Laboratory measurements should be entirely dissociated from workshop measurements, both as regards the accuracy aimed at and the methods employed.

Mr. WILLANS: All that can be said about the electrical part of the arrangement has probably been said; but it may not, perhaps, be out of place, as I have nothing whatever to do with the arrangement of the instruments, for me to say a few words about the need for them. No other credit is due to me than that I pointed out that something of the sort was much wanted, in consequence of the discrepancy between commercial instruments. I myself began with great faith in the ordinary electrical instruments; but, after taking readings sometimes with one and sometimes with another instrument, I began to lose it. Of course we are not working for ourselves, but mainly for dynamo makers, and I began to find, whatever faith we had in the instruments we selected, other dynamo makers had not confidence in them. They had great confidence in their own instruments, as a rule, but not much in others provided for them; at any rate, this was very frequently the case. We ourselves always had worked with Siemens instruments, and our results altogether need practically no correction, as shown by the agreement between our earlier results and those since we adopted standard instruments. The only difficulty with the Siemens dynamometers is the adjustment of the zero. The adjustment of the zero is a troublesome thing in the workshop, especially in the large-current dynamometers. We have to measure the current when it is not a fixed quantity,

Mr.  
Williams.

as in the case, for instance, of 300 H.P. absorbed by iron wire resistance on a windy day. We do not deal with a uniform current in our ordinary tests, but we are measuring one which is varying to some extent from one minute to the next. A balance which takes time to adjust, such as a sensitive dynamometer, or a potentiometer on Mr. Crompton's principle, is very much like a large platform weighing machine. If we place a horse and cart on a machine of this description, and keep it standing there, we can ascertain its weight with great exactness; but in measuring the amperes we are measuring in many cases a quantity which is varying all the time: it is as though we tried to weigh the carts and horses as they passed over the weighing machine. For this purpose a delicate balance is the worst instrument possible.

We want an instrument which will enable us to get at a glance the weight at any moment, and then we can average it. We can get any number of people who can say they have gone through the balancing process with the potentiometer. The younger a man is the quicker he gets through that sort of measurement, and the more places of decimals he can balance to; but I have not great confidence in these measurements unless made by experienced men. Moreover, two people cannot use the balance at the same time. When you have the spot of light, to which objection has been taken, at least it is a spot which two people can see, and two people can make a note of its position. For instance, we can see it, and our customer can see it—that is, so far as the position of the light goes: it may be that it is not in the right place, but still it is somewhere. Having ascertained that that spot of light is, on the average, in a certain position for half an hour, the method Captain Sankey has suggested, as it seems to me, is to provide a ready method of ascertaining what that average position really means—a method which can be applied in a few moments. Mr. Crompton takes a running shot at it, so to speak, with the Clark's cell, and Professor Ayrton trusts to the scale deflection meaning always the same thing. Captain Sankey, it seems to me, combines the advantages of both methods. He gets

the average reading more correctly than Mr. Crompton with variable currents, and he gets the absolute check more perfectly and promptly than Professor Ayrton. The effect generally of introducing the absolute method of measurements has not been great on our efficiencies, but our highest efficiencies have gone down about 1 per cent. The reason for this may not be apparent to everybody, but it is a simple one. Suppose the real results to be indicated by points on a horizontal line, and suppose in successive tests the apparent results are, on account of errors in instruments, some 1 per cent. above the line, and some 1 per cent. below—no one talks of the ones which are below the line; they only talk about the maximum efficiency, which is generally with uncertain instruments above the real one—and the awkward thing for us is that everyone who has once got one of the high results wants it again, and will not be satisfied unless he always gets an abnormal efficiency; so that it has been a relief to me to get some means which will ensure a correct result. Of course Captain Sankey himself will explain why the d'Arsonval galvanometer was used; the great difficulty in the workshop is that we have magnetic fields all over the place: the great majority of instruments will not read right at all under such conditions; you cannot depend upon them. Many instruments that are quite right out of the way of iron and currents are easily upset to the extent of 5 per cent. or more, as in the workshop, by stray fields and by currents passing.

In conclusion, I cannot say too much in praise of the very careful methods employed by Mr. Andersen in arranging the apparatus designed by Captain Sankey. I have watched him with great interest, and have seen that he has taken nothing for granted, but checked again and again every little point; and the result is, at any rate, an apparatus which gives consistent results, and, as has been shown by comparison with other standard instruments, correct ones also.

Captain SANKEY: At this late hour my remarks must be very short. The principal criticisms come from Professor Ayrton, and the main difference between his method and ours is simply this—that for accurate measurements he depends upon the scale of

Mr.  
Willans.

Captain  
Sankey

captain  
Sankey.

the galvanometer, and we do not. We only use the scale for *ordinary* measurements. For *accurate* measurements, such as are required for efficiency tests, we obtain the mean deflection by taking readings at frequent intervals—"spot watching," as Mr. Swinburne calls it—and afterwards this mean deflection is calibrated. In this manner several sources of error are cut out, and, amongst minor ones, changes in the length of the scale; and it would be interesting to know from Professor Ayrton what is the percentage of error due to variations in the material of the hexagonal roller on which his scales are mounted. It also cuts out any error due to secular changes and to irregularities in the law of deflection of the galvanometer, so that we are not dependent for accuracy on the scale of the galvanometer. Although not necessary for us at Thames Ditton, we are much obliged to Professor Ayrton for having described the arrangements which enable him to obtain a straight line for the law of deflection, and *apparently* to eliminate secular changes. Another important point of difference between the two methods is the manner in which the resistance through which the current passes (resistance B in our case) is measured. Professor Ayrton depends upon the copper-deposition method, and instead of that we have adopted the method of using the standardising resistance, A, as a step to the measurement of resistance B, as described at page 530. Of course the copper-deposition method is looked upon by some people as being the more accurate way, but for several reasons I do not think it is. However that may be, I feel perfectly sure it is not a method suitable for the engineer's workshop, and for these reasons this method was rejected when the apparatus was being designed. Professor Ayrton corrects me, and says he did not recommend the copper method for use in the workshop; if so, we would have to depend on someone else for our calibration, and our condition No. 1 would not be fulfilled. Some means of calibration are evidently necessary, if only on account of the doubt respecting platinoid as to whether its resistance does not change with time. Nobody knows much about it yet at any rate. We have therefore adopted a method which enables us

at any moment, by simply altering a few connections, to calibrate by comparison with the standard ohm and the standard cell. Our standard ohm can be, and has been, compared at Cambridge, and later on at the Board of Trade, when they are ready; and similarly with the standard cell. Professor Ayrton asked why we did not employ a low-resistance galvanometer wound with platinoid for measuring currents. Resistance B was so designed as to make it unnecessary to have a galvanometer of low resistance, and it was therefore possible to have both galvanometers (for current and for volts) the same, which has several practical advantages. If the galvanometer were wound with platinoid it would be far less sensitive; but I will leave further remarks on that subject to Mr. Andersen.

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Sankey.

Mr. Evershed is surprised we made no mention of certain temperature corrections due to the galvanometers; but I think it will be seen that by the method we have adopted for *accurate* measurements we cut out all those kinds of errors—this was, in fact, one of the reasons for adopting the method. These temperature errors are, of course, insignificant for ordinary workshop readings. He also asked where the potential wires were connected in resistance A. This resistance is not used for measuring large currents. But as regards resistance B, where the question is of importance, the conducting leads and potential wires were very carefully arranged so as to get a cross section where there is uniform density of flow.

Mr. F. V. ANDERSEN: The principle of the measurements described in the paper is this—that when the values of the mean readings of amperes and volts obtained during a trial is required to be as accurately known as possible, then this value is ascertained at the end of the trial by specially calibrating the reading; and this is the best way of getting really great accuracy, because you thereby carry the measurements back to the standard ohm and the standard cell. As regards the standard ohm, no electrician has much doubt that that comes within the Board of Trade requirements in regard to accuracy. As regards the reliability of the standard cell, I think Mr. Swinburne has more apprehension than is necessary. Mr. Swinburne admits that you can get cells to

Mr.  
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Mr.  
Andersen.

compare well with each other; but he thinks that there is considerable difficulty in hitting the normal value—the value determined by Lord Rayleigh. Now I would mention that, from experience with those cells for a considerable number of years, I know that you will succeed in reproducing the original value when you have found the proper way of making the cells. Dr. Muirhead, for instance, and Mr. Herbert Taylor possess cells many years old, which agree not only between themselves, but also with the cells made now. If the profession would procure their cells, at least for some time, from experienced makers, I have no doubt that the same confidence would be felt in regard to the standard cell which exists in regard to the standard ohm. I have found, by calibrating galvanometers at different times and at very different temperatures, that the temperature coefficient of the cell is pretty reliable. It is well known that that coefficient is not exactly constant from very low temperatures up to high ones, but for ordinary temperatures the use of the coefficient leads to only very small errors.

The next question is about the use of the spot of light in the workshop. I may say I was rather surprised to hear from three or four important sides that the spot is not likely to gain favour with engineers. I am glad that the spot has given satisfaction in at least one place where it has been tried by engineers. My own impression about this method of reading is, that it is simply charming. No oil lamps are wanted now; you turn on the electric light, and if you fix the instrument in its place after you have calibrated it, I certainly cannot see why the spot of light should be objected to, being far superior in the facility with which you read it to any pointers.

I should like to make a few remarks on the design of the d'Arsonval galvanometer. I agree with Mr. Crompton that improvements are badly wanted in this important instrument, and I have been very glad to see some of Professor Ayrton's refinements in the construction of this instrument. Professor Ayrton has explained a number of improvements which I think are very good. In one point, however, I do not agree with Professor Ayrton—that is, in regard to the winding. In a system like the one

before you, rightly called a calibrating system, it is rather important on many occasions to have a sensitive galvanometer. But I do not see how it is possible to get the present d'Arsonval galvanometer sensitive enough if you wind it with platinoid wire. Whatever sensitiveness you can get with the platinoid wire will be exceeded no less than 25 times by winding with copper wire, because the torque obtained in a given magnetic field is inversely proportional to the specific resistance of the wire with which the instrument is wound. As shown, the present system depends on accurate balancing of resistance and electro-motive force. One would require a Thomson galvanometer for these purposes, if the d'Arsonval galvanometers were wound with platinoid wire. If for no other reason, it may prove in the future that copper-wire winding will be adhered to. There can be no doubt that Professor Ayrton's design with soft iron pole-pieces is good, and also the suspension with a flat strip of metal from the top, and a spiral at the bottom—which avoids putting a strain on the coil, and leaves it free to take its position by gravity—the latter especially—is an important improvement. I myself used it several years ago, and found it very practical. The flat-strip suspension I adopted in ammeters and voltmeters in 1883. As regards the magnetic shunt, it is a very beautiful thing, but I doubt if it is useful for engineers.

Then as to the six scales. It is a very ingenious arrangement. You turn the drum and get another scale, changing at the same time the resistance. I do not, however, think it necessary to go to that refinement. We find that people can be trusted to adjust the resistance in the ordinary way. Nor is more than one scale required. When this scale is suitably calibrated, it can only be five extra sets of figures you want, and not five more divided scales. It was mentioned that the platinoid-wire winding would cut out the temperature corrections altogether. It will not quite do that, because there is one temperature correction which it is difficult to get rid of—that is, the correction for the rise in temperature in the platinoid rheostat carrying the main current.

Mr.  
Andersen]

Professor  
Ayrton.

Professor AYRTON: That is allowed for. These pieces are calculated so that there is no correction wanted.

Mr.  
Andersen.

Mr. ANDERSEN: We have found that with strong currents this rise of temperature is considerable, even with a low current-density, so that a correction is necessary.

Captain  
Sankey.

Captain SANKEY: Allow me to add that the apparatus quite admits of being calibrated by the copper-deposition method, and I shall be very glad if Professor Ayrton will come down to Thames Ditton and calibrate it by that method.

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Sankey  
and Mr.  
Andersen

Captain SANKEY and Mr. ANDERSEN [*communicated*]: Having had the opportunity of seeing the remarks of the various speakers in print, we have been able to go into the matter more fully, and desire to make the following further remarks; but in the first place we must thank Prof. Ayrton for the care and trouble he has taken in criticising the paper, and as regards his remarks respecting d'Arsonval galvanometers, the various points he mentions will be fully taken into account in the new galvanometers it is proposed to obtain, as mentioned in the paper at page 530. Prof. Ayrton's method, and the one he wishes us to adopt, is to read the amperes or the volts direct from the scales of the d'Arsonval galvanometers. Now, apart from the method of calibration—which is, of course, a distinct question—this is precisely the method we use for what we have called “ordinary” measurements as distinct from “accurate” measurements, such as are required, for instance, for efficiency tests; but we do not rely on the scale readings giving results nearer than 0·3 per cent. on the average, or even 0·5 per cent. in extreme cases, because of possible fluctuations in the sensibility of the galvanometers, and of various errors due to temperature (such as mentioned by Mr. Evershed), and we fail to see sufficient provision in Prof. Ayrton's arrangements for dealing with such errors. It therefore appears to us that his method only compares, on the point of accuracy, with our scale readings for “ordinary measurements.” Of course, if he can read the scale to within 0·1 per cent., so can we; but it does not follow that the reading either of amperes or of volts is within 0·1 per cent. of the true value. It is to be

observed in this connection that when purely *electrical* measurements are required it is not so essential to obtain absolute values ; but when making efficiency tests, in which electrical output has to be compared with indicated power, or with a weighed quantity of water, the degree of accuracy of the electrical readings must be referred to *absolute* standards, and we doubt whether Prof. Ayrton's readings are within 0.1 per cent. of the absolute values.

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For efficiency test measurements we therefore prefer the calibration method described in the paper (formulae 2 and 4), which, it will be observed, cuts out all questions of change of sensibility of the galvanometer, due either to time, to temperature, &c.

Prof. Ayrton says that "if the coil in the d'Arsonval galvanometer be suspended with a strip of phosphor-bronze, there is "not much chance of the sensibility of the instrument changing" (page 550). This opinion appears to be based on the two measurements obtained with the "ammeter d'Arsonval galvanometer" on October 2nd, 1889, and January 30th, 1891 (page 545). These two results are, in the first place, tainted with any error in the measurement of the 120 amperes passing through the platinoid sheet ; secondly, with any change in the resistance of the platinoids sheet ; and thirdly, what evidence is there that the apparent constancy of the sensibility is not due to a fortuitous coincidence of the changes in the magnets and in the phosphor-bronze suspension ? At any rate, until far more indisputable evidence is forthcoming that a d'Arsonval galvanometer can be so constructed that its sensibility will remain *absolutely* unchanged over considerable periods of time, it is surely wiser (for "accurate" measurements) to adopt a method independent of the sensibility. Prof. Ayrton, however, does not himself rely on the sensibility remaining unchanged, and therefore provides a magnetic shunt, which of necessity requires some means of determining the sensibility of the galvanometer at any time after the apparatus has been established in the workshop. He does not say how he proposes to do this, but presumably he would use a Thomson balance, judging from his remarks at page 561. We would, however, prefer to do it by the method described in the paper at

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and Mr.  
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page 526, because it is easier to make the measurements, and the comparison can be made direct with the standard cell. It is also to be observed, with all due deference, that the use of a Thomson balance does not fulfil our condition No. 1, and would add considerably to the cost of the apparatus.

The use of standard Thomson instruments by themselves is no doubt an excellent method, but their combination with the method advocated by Professor Ayrton—which method can be so readily transformed into a standardising arrangement similar to the one at Thames Ditton—does not really appear rational. As a somewhat important point, what guarantee is there that the magnetic shunt has not been interfered with without the tester's knowledge?

Professor Ayrton has pointed out that in the case of the constant 0.05 in Table II., the use of the potentiometer rheostat leads to an error in the scale reading of 0.4 per cent. It is to be observed that this error occurs, however, only when measuring potential differences of from 1.7 to 8 volts (not in the neighbourhood of 0.05 volt as stated by Professor Ayrton), and is the greatest error occurring from this cause. The next greatest is 0.25 per cent. with constant 0.1, but for the really important readings in the range from 20 to 700 volts the maximum error from this cause is 0.04 per cent. Moreover, these errors only apply to the "ordinary" measurements, which are not considered to be nearer than from 0.3 per cent. to 0.5 per cent., as stated in the paper.

Originally the magnitude of this source of error was calculated for the range 20 to 700 volts, and when it was found to be only 0.04 per cent. it was unfortunately hastily assumed that the error would be likewise insignificant for other ranges, and we are therefore much obliged to Professor Ayrton for having called our attention to the matter, and we now find that a simple correction to the resistances used in series with the galvanometer (Table II.) cuts out this class of error from the direct scale readings. If the resistance used in series with the voltmeter galvanometer when the potentiometer rheostat is used is taken as  $\frac{f}{k} \cdot K - (G + r_a)$ , instead of  $\frac{f}{k} \cdot K - G$  as in equation 3, then the error in question

is practically eliminated. As will be seen, the effect of introducing this correction in Table II. is to diminish  $R_e$  in that part of the table referring to measurements made with the potentiometer rheostat by an amount  $= r_a$ , or 2.5 ohms. The table in use at Thames Ditton has been thus altered.

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Sankey  
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As regards the supposed error of 1 per cent. in connection with constant 0.0005, Professor Ayrton has omitted to observe the effect of the note at the bottom of Table II., which exactly accounts for the source of error he thinks he has discovered.

The criticism has, however, no bearing whatever on the question whether or not an accuracy of one-fifth per cent. is obtained with the apparatus, because this source of error *does not occur at all* when using equation 4 for "accurate" measurements; in this case the ratio  $\frac{r}{r_a}$  is found by actual measurement with the Wheatstone bridge, and in measuring  $r_a$  the galvanometer circuit is left closed across the small section of the rheostat.

We now come to the question of calibration. As regards volts, it is evident that practically it must at present be based on the standard cell. As regards amperes, however, the comparison can be made either by the voltameter method, or by measuring the fall of potential (compared with the standard cell) across a resistance measured by comparison with the standard ohm. We prefer the second method, but Professor Ayrton prefers the first. He doubts our method because of the difficulty of measuring very small resistances with sufficient accuracy. Of course it is difficult (Mr. Swinburne notwithstanding—see p. 568), but it can be done; and as the probable error of resistance B, as obtained from the results of the periodical calibrations of this resistance, is 0.05 per cent., there does not appear to be any room for doubt but that we have succeeded in doing so at Thames Ditton.

Professor Ayrton says: "The best way of measuring a small resistance for a big current passing through it is to send a large known current through the resistance and measure the P.D. at its terminals." The principle here recommended is (with the difference in the method of measuring the "large known current")

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exactly the one used at Thames Ditton for measuring the resistance of B, and described in the paper in section 3, "Measuring "Amperes," and in section 6 under the head "Main Ampere-  
"Rheostat." Referring to equation 7, it is clear that  $\frac{A}{e}$  is the reciprocal of the known current, A being the resistance of the standardising rheostat when placed in parallel (*i.e.*, 0.029071 ohm at 11.4° C.), and  $e$  the E.M.F. of the standard cell. Further,  $c$  ( $G_1 + R$ ) is the measured P.D. across the terminals of resistance B.

The apparatus at Thames Ditton, however, permits of the resistance of B being measured in a different manner, as will be seen by the following, in which any *constant* E.M.F. can be substituted for that of the standard cell and a *constant*, but unknown, resistance for A. We have from equations 8 and 5,

$$A = S \sqrt{\frac{R_2}{R_1}},$$

and

$$c = \frac{e}{mR + G}.$$

Inserting these values in equation 7, we get, adding the suffix  $b$  to the  $R$  of equation 7, to distinguish from the  $R$  of equation 5,

$$B = S \sqrt{\frac{R_2}{R_1}} \cdot \frac{(R_b + G)}{(mR + G)}.$$

Thus B is obtained by multiplying the resistance, S, of the standard ohm into two ratios of resistances, and these ratios can be ascertained with an accuracy of 1 in 10,000.

Professor Ayrton, after criticising the use of mercury cups in connection with resistance A, says: "The buyer takes it for granted that he knows a resistance which is only about the one-thousandth of an ohm to something like the one-five-hundredth-thousandth of an ohm, since that is what is necessary if 'the errors to which the measurements are liable should not exceed one-fifth per cent.' And apparently the buyer has had such a belief in the goodness of the steam engines manufactured by Messrs. Willans & Robinson that, on seeing the resistance of five strips of platinoid measured when in series, he has assumed, in spite of the mercury cups, that he could

“predict the exact resistance of these strips when put in parallel to one-five-hundredth-thousandth of an ohm.” (The italics are our own.) Now the resistance consisting of five strips, and fitted with mercury cups, is resistance A (see “Apparatus Required,” p. 517—“1 standard rheostat,” &c., and also p. 531, “Rheostat A”), and its resistance, when in series, is 0·72659 at 14·8° C.; so that for one-fifth per cent. of accuracy it need only be measured to 0·00145 ohm, and not to 0·000002, as stated by Professor Ayrton. As before observed, however, one-fifth per cent. is the accuracy aimed at in the determination of amperes as compared with absolute values, and for this A must evidently be known much nearer than one-fifth per cent., and the probable error of one determination is 0·007 per cent. The fact is, Professor Ayrton has confused resistance A with resistance B, consequently his criticisms on mercury cups, screw connections, &c., do not apply, in that nothing of the kind exists in resistance B: everything is most carefully soldered, and the current is distributed in the manner mentioned by Professor Ayrton.

The objection to mercury cups is, of course, that their resistance is liable to change. Now in the case of resistance A, whatever the resistance of the mercury cups may be, it is measured and taken into account when comparing B *immediately* afterwards. It was shown in Captain Sankey's further remarks that 1·25th of the resistance of A in series is  $\frac{1}{5} s + \frac{2}{10} m$ , and that the resistance in parallel is  $\frac{1}{5} s + \frac{1}{5} m$ . It may be objected that the  $m$  in the first case has not the same value as the  $m$  in the second case, seeing that different mercury cups come into use in each case. We have therefore measured the resistance of a number of these mercury cups, and find that it varies from  $\frac{45}{10^6}$  to  $\frac{120}{10^6}$  ohm. The *greatest* possible error is therefore evidently  $\frac{1}{5} \left( \frac{120}{10^6} - \frac{45}{10^6} \right)$ , or 0·05 per cent., which would occur if all the mercury cups when in series had the higher resistance, and all those when in parallel the lower resistance, or *vice versa*—of course, a most unlikely arrangement. We think, therefore, that

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our contention that A can be measured in series and used in parallel is well established. Professor Ayrton says: "But if the only way of ascertaining this fact is by an actual measurement of their resistance in parallel, why put the strips in series at all to measure their resistance, and why use any mercury cups and copper bridge-pieces?" The above, we think, shows that there is another way of ascertaining this fact; but as regards the latter part of the sentence, we have to observe that at the time resistance A was designed the method of comparison with the standard ohm described at p. 533 had not been thought of, and it is really this arrangement which has enabled us to measure a resistance as low as 0.02907 with an error certainly not more than 0.1 per cent. On the other hand, the arrangement of A with mercury cups is convenient at Thames Ditton for other purposes. Whereas, therefore, we see no objection to the use of mercury cups in an *intermediate* resistance like A, they are not in any way necessary to the system of measurements.

As there has been misunderstanding on the point, we repeat that A is not used for measuring large currents, but that B is used for this purpose.

Professor Ayrton raises the question of "faith." As far as we are concerned, the faith extends only to the standard ohm and the standard cell. No other resistances were accepted; they were compared with the standard ohm. In conformity with condition 1 (page 517), it was not possible to extend the faith even to a Thomson balance.

Professor Ayrton himself apparently does not entirely rest on his faith in the Thomson balance, but uses a "copper-deposit check" on this instrument. He says, however, that such a check "would be quite unnecessary at a works like Thames Ditton." We do not know why this check is considered "quite unnecessary;" we, however, think it is quite unsuited, and for the following reasons:—To deposit copper with the standardising current of 50 amperes requires a considerable depositing surface, which means large plates. These plates have to be weighed twice after very careful washing and drying, and the balance used must be capable of weighing a considerable weight with

great accuracy. The weight of copper deposited is the *difference* between these two weighings, and the percentage accuracy of the determination is of course considerably less than the accuracy of one weighing, especially as the errors in the weight may be in opposite directions. The depositing current must be kept constant from the beginning to the end of the test, which is not easy to do without a special device, and the duration of the test—that is, from the *instant* of closing the circuit to the *instant* of opening it again—must be accurately timed. Great care must also be exercised with regard to the solution and to the purity of the copper. It is to be observed that any slip invalidates the result past recovery. We think Prof. Ayrton's students are much to be congratulated that they are even "beginning to get successive determinations of the constant" thus obtained to agree to 0.06 per cent.

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As a minor point, Prof. Ayrton says: "A little consideration, however, will show that the authors have not, with the apparatus they have arranged, fulfilled the conditions they started by imposing on themselves. . . . Measuring a current of 1-40th ampere to one-fifth per cent. would necessitate, with the apparatus they employ, that the deflection should be read accurately to the one-thousandth of a millimetre." Now resistance A is part of the apparatus, and, of course, would be used instead of B to measure small currents, and for 1-40th ampere would be placed in series. To obtain an accuracy of one-fifth per cent. a reading of 500 on the *ampere* scale is required, and the constant should therefore be 0.00005. Hence, according to equation 1, but replacing B by A (= 0.7266), we have for the resistance to be placed in series with the galvanometer,

$$R_A = 0.00005 \frac{A}{0.06} 10^6 - 429 = 176 \text{ ohms.}$$

It follows, therefore, that 1-40th ampere can be measured by the apparatus with the specified accuracy of one-fifth per cent.

Professor Ayrton concludes his criticism with some suggestions of improvements in the apparatus, which are said to be due to Mr. Mather. As has already been explained, the proposed platinoid galvanometer would not enable the various

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measurements involved in the system to be made with sufficient accuracy, and the proposed platinoid sheets, being 0·4 inch thick, would, according to our experience, so far from doing away with the necessity of temperature corrections, increase the present ones. It is not *thick sheets* that are required for this purpose, but *thin strips*, in order to provide abundance of cooling surface for the energy given to the rheostat. Another objection to thick sheets is that the interior will be warmer than the exterior. This will alter the distribution of the current, and thus increase the resistance of the platinoid sheets.

A further disadvantage of the proposed plan is that the galvanometer would work with comparatively strong currents, and fresh errors would be introduced by heating in the resistance coils used in series with the galvanometer, by small resistances in the contacts of the key, &c.

Mr. Swinburne has said that he prefers a commercial voltmeter or ampere-meter to a reflecting galvanometer, and that if the former are not accurate, then all that is necessary is to calibrate them. On this point we have to say that such a process formed part of the scheme when the system under discussion was introduced at Thames Ditton. Messrs. Willans & Robinson possess a great number of the best commercial voltmeters and ammeters from various makers. It was found that when these instruments were placed sufficiently far away from the engines and dynamos, and tested by the new apparatus, the accuracy of their calibration left very little to be desired: in almost all cases they would measure accurately to within three-fourths of 1 per cent. But when they were placed near the steaming bed, where the dynamos are tested, then discrepancies were discovered up to 3 and even 5 per cent.; and it is simply impossible to make sure of the results under these circumstances, as the errors alter when a new dynamo is started or stopped. Naturally, therefore, the commercial instruments are now used only for the purpose of indicating that the load on any particular dynamo is roughly correct, whereas the readings for measuring purposes are taken on the standard instruments.

Reference has already been made (by Mr. Andersen) to the

main point in Mr. Trotter's remarks, viz., the question whether the d'Arsonval reflecting galvanometer is a suitable instrument for workshop use. But, as the question is of great importance, we should like to add a few words to what we have already said. Apart from the fact that all ordinary electro-magnetic instruments are liable to be affected seriously in their indication when used in the vicinity of dynamo machinery in action, another difficulty arises when, in the course of the test of a dynamo at varying output, you step down from a stronger to a weaker current: then the deflection on the electro-magnetic instrument does not exactly follow, but decreases less than the corresponding current. It would therefore be impossible at the present time, even if we shared Mr. Trotter's opinion that an instrument with a pointer is preferable to a reflecting galvanometer, to have fixed on the use of any particular type of commercial instrument, quite irrespective of the fact that some kind of standard such as Siemens's dynamometers or Sir William Thomson's balance would under any circumstance be required to check the commercial instruments. But coming now to the question of the best arrangement for taking readings, we do not think that engineers who have once worked with a good d'Arsonval galvanometer will ever part with it in favour of instruments with pointers. Readings can, of course, be taken to greater accuracy with the spot than with a pointer: the sensitiveness of the reflecting galvanometer is absolute; the dead-beatness may be almost perfect; and the range, when using the arrangement with resistances in series with a sensitive galvanometer, is easily made to cover the whole of the requirements of a shop by means of a *single* instrument. These are advantages which you cannot equal with the ordinary commercial instruments.

We agree with Mr. Crompton that the null method is more accurate, but only so long as the current to be measured is *steady*. For this reason we have used the null method to measure current when comparing resistance B with resistance A (see page 530); when the current is *unsteady* (however little), we prefer the deflectional method, and by calibrating the mean reading we gain the advantage claimed by Mr. Crompton for the null method

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when he says that "no disturbance or interference with the  
"instruments is likely to cause unsuspected errors."

Mr.  
Siemens.

The CHAIRMAN: There is one feature about this paper not alluded to by anybody except briefly by Mr. Trotter, viz., that when this Institution was founded electrical engineers measured by miles of iron wire and by Daniell cells, and I think this paper affords a proof of how much the profession of electrical engineering has since become a "science." If we are able to attain an accuracy of one in ten thousand, it certainly shows that profession to be working in a very scientific manner, and I doubt if any other branch of engineering can show so much progress in 20 short years.

I will now ask you again to give our best thanks to the authors of the paper, and to those who have contributed to the discussion.

The following candidates were declared by the scrutineers to be duly elected:—

*Foreign Members:*

Auguste Bonel.	Thomas Commerford Martin.
O. T. Crosby.	Paul Roux.
William Dierman.	Nikola Tesla.
John Henrik Hammar.	Joseph Wetzler.

*Members:*

James Thomas Baron.	Lee L. Murray.
James S. Fitzmaurice.	Thomas George Poole.
Charles James Hall.	Stuart Arthur Russell.
H. H. Kingsbury.	Malcolm Sutherland.
Major Percy A. Macmahon, R.A., F.R.S.	William White.

*Associates:*

William Armistead.	Edward Burn.
Frank Boulton Aspinall.	Ernest John Clapp.
James T. Auchinachie.	Thomas Cockerill.
Gustav Adolph Boettger.	William W. Cook.
E. W. Bramble.	William Arthur Cox.
Harry Buchanan.	H. K. Dando.

A. A. Dircks.	James Bennet Peace.
William Edgar.	E. Jesse Piper.
Charles Henry Fleetwood.	Joseph Poole.
Frank Gill.	Hugh L. Randolph.
Stuart Mortlock Hancocks.	Adolphus John E. Richardson.
Fred. Harrison.	Oliver A. Richardson.
Henry J. James.	H. Ritchie.
B. Holmes Jenkinson.	Ferdinand Saunders.
Ernest Kingsbury.	Adolph Schneider.
Percival Phillip Kipping.	Ernest Scott.
E. A. Langeschwerdt.	Frederick Joseph Sissison.
John Mackail.	Arthur Spain.
James McPherson.	Frank Tandy.
J. Y. Nelson.	H. K. Tavaria, B.Sc., L.M.&S.
Edmund L. O'Brien.	Chris. T. Tralen.
Henry Charles Parsons.	Robert Alfred Wormell.

*Students :*

Herbert Sydney Austin.	Arthur John Hodgson.
Tom Treherne Barton.	William Holmes.
Knud Bentzen.	Nai Khem.
Herbert Henry Berry.	Herbert David William Lewis.
Harry C. Channon.	Sydney Weiss Mitchell.
William James Davy.	Harry Frederick Nutter.
Edward Winram Dickinson.	Arthur T. Smith.
William Dickinson.	Hastings Squire.
Albert Edward Firmstone.	Charles Edward Tickner.
Cecil Charles Fowler.	Alan H. Tripp.
Henry Joseph Garnett.	Ernst Carl Uldall.
John Clements Good.	Louis Heathcote Walter.
John Rothwell Drake Goodban.	

The meeting then adjourned.

The Twentieth Annual General Meeting of the Institution was held at the Institution of Civil Engineers, 25, Great George Street, Westminster, on Thursday evening, December 10th, 1891—Professor W. CROOKES, F.R.S., President, in the Chair.

The minutes of the Ordinary General Meeting held on Thursday, November 26th, 1891, were read and approved.

The names of new candidates for election into the Institution were announced and ordered to be suspended.

The following transfers were announced as having been approved by the Council:—

From the class of Associates to that of Members—

Gustav Binswanger.	G. F. Preston.
Edward Woodrowe Cowan.	John Hall Rider.
W. P. James Fawcus.	Francis M. Rogers, F.C.S.
John Hayton Greenhill.	J. W. Ullett.
Francis John Mudford.	Julian Elton Young.

From the class of Students to that of Associates—

Donald Strode Barton.	Arthur G. Seaman.
Archie Davidson.	Percy Woodward Snelus.
Noel Crossley Jones.	

The PRESIDENT having announced that the ballot for Council and Officers for 1892, and for the election of new members, would close at 8.30, Messrs. Richard Aylmer, G. Binswanger, R. W. Blackwood, and W. H. Patchell were appointed Scrutineers.

The SECRETARY then read the Annual Report of the Council, as follows:—

### REPORT OF THE COUNCIL.

The Council have satisfaction in reporting that the increase in the number of members since the last Annual Report has been very large.

Inclusive of the candidates to be balloted for this evening, 22 Foreign Members, 31 Members, 154 Associates, and 137 Students—making a total of 344—will have been added to the

register during the year, besides Mr. Jacob Brett, whose election by the Council as an Honorary Member was recently announced.

The eminent services rendered to telegraphic science by Mr. Brett were so ably and eloquently set forth by your Past-President, Mr. Latimer Clark, when the election was announced, that further allusion to them is superfluous; but members will be pleased to know that the veteran telegraph engineer has expressed great gratification at his election, and at the marks of approbation with which the announcement was received by them.

47 Associates have been transferred to the class of Members, and 26 Students to the class of Associates.

Our losses by death during the year include 1 Honorary Member, 1 Foreign Member, 8 Members, and 3 Associates, making a total of 13, which, taking into consideration our increased numbers, is fortunately not above the average; but among those taken from us are, unhappily, several whose names were prominent on the register. The list comprises Professor Wilhelm Eduard Weber, Honorary Member, who was the associate of Gauss in the establishment of the electro-magnetic system of measurement, and who developed the first definite system of electric current measurement. There are few branches of electrical science which have not been enriched by Professor Weber's researches. The Royal Society's catalogue of scientific papers contains the titles of no fewer than 59 of his papers, published between the years 1825 and 1863. We have also lost Mr. Willoughby Smith, Past-President, whose labours and researches in connection with many branches of applied electrical science have proved of great value to the electrical engineer; Messrs. W. Lant Carpenter and H. G. Erichsen, former Members of Council; Colonel Sir Oliver W. St. John, and Messrs Louis Crossley, E. Gilbert, W. M. Shaw, and J. H. Soll, Members; M David Brooks, of Philadelphia, Foreign Member; and Messrs. Douglas Gibbs, George Müller, and H. R. Rich, Associates.

3 Foreign Members, 6 Members, and 15 Associates have resigned during the year.

The attendance at the Ordinary General Meetings has been very large, and it is fortunate that the liberality of the Institution of Civil Engineers enables us to hold them in this spacious hall.

In addition to the brilliant Inaugural Address of the President, the following papers have been read, and have in most cases given rise to important discussions :—

LIST OF PAPERS READ BEFORE THE INSTITUTION DURING THE  
YEAR 1891.

DATE.	TITLE.	AUTHOR.
Jan. 22.—	Electric Lighting from Central Stations, with Special Reference to the Chelsea System ... ..	Major-Gen. C. E. WEBBER C.B. (late R.E.), Past- President.
Feb. 19.—	Transformer Distribution ... ..	JAMES SWINBURNE, Member.
April 9.—	Notes on the Design of Multipolar Dynamos	W. B. ESSON, Member.
„ 23.—	A Few Calculations on Electrical Shocks from Contact with High-Pressure Con- ductors ... ..	Major P. Cardew, R.E., Member.
May 7.—	On some Effects of Alternating-Current Flow in Circuits having Capacity and Self-Induction ... ..	Dr. J. A. FLEMING, Mem- ber.
„ 7.—	On some Points connected with Mains for Electric Lighting ... ..	W. H. PREECE, F.R.S., Past-President.
„ 14.—	On the Most Economical Mode of Feeding a Low-Pressure Network ... ..	R. E. CROMPTON, Vice- President.
Nov. 12.—	Description of the Standard Volt- and Ampere-Meter used at the Ferry Works, Thames Ditton ... ..	Capt. H. R. SANKEY, R.E., Member, and F. V. ANDERSEN, Associate.
Dec. 10.—	On the Specification of Insulated Conductors for Electric Lighting and other Purposes	W. H. PREECE, F.R.S., Past-President.

In respect of papers contributed by Members or Associates not on the Council, and which have been read or published during

the twelve months ending the 31st of May last, the Council have awarded the following annual premiums :—

*The Institution Premium*, value £10, between C. G. Lamb, E. W. Smith, and M. W. Woods, Associates, for the two papers by Professor Ayrton and themselves on “Secondary Cells.”

*The Paris Electrical Exhibition Premium*, value £5, to James Swinburne, Member, for his paper on “Transformer Distribution.”

*The Fahie Premium*, value £5, to P. V. Luke, C.I.E., Member, for his communication, “The Early History of the “Telegraph in India.”

#### NEW OFFICES.

The removal of the Library, &c., to 28, Victoria Street, was effected in January last, and the new offices have proved to be much more convenient in many respects than those formerly occupied by the Institution. Some considerable expense was incurred by the removal, and by the necessity of altering the book-cases, as well as by the purchase of additional furniture; but the Council have reason to believe that when the yearly accounts are made up, it will be found, owing to the large number of members elected this year, that this extra expenditure will have been met without the necessity of drawing on the reserve fund.

#### ALTERATION IN THE RATES OF SUBSCRIPTION, &c.

The Council, being of opinion that the time had arrived when the entrance fees and annual subscriptions of future members should be raised, as indicated in their last Annual Report, submitted a proposal to that effect in the form of a special resolution, which was unanimously passed and confirmed at Special General Meetings of members held in August last. The higher scale of fees will apply to all members elected after the 31st inst.

#### BENEVOLENT FUND.

In the belief that it is very desirable to establish a Benevolent Fund in connection with the Institution, the Council issued in April last a circular to all members upon the subject, inviting subscriptions to such a fund; and although it has not been so fully responded to as they had hoped, a sum of £394 6s. 6d. has

been subscribed, which they hope will be largely increased before any necessity arises for the fund being called upon.

#### THE SALOMONS SCHOLARSHIP.

The Council have given very careful consideration to the conditions which should govern the award of the scholarship so liberally founded by your Vice-President, Sir David Salomons; they have been desirous of choosing a course between offering the award to an inconveniently wide field of candidates, and, on the other hand, of limiting that field unduly. They have finally decided, with the full approval of Sir David Salomons, that the four following colleges where electrical engineering is taught—viz. :

The City and Guilds of London Central Institution,

The City and Guilds of London Technical College, Finsbury,  
King's College, London,

University College, London,

shall be invited to recommend each year for the scholarship, one of their students who has attended for two years the classes included in the course for electrical engineering at their respective colleges, and who intends to follow the profession of electrical engineer, to enable him to continue his technical studies for another year.

From the candidates so nominated the Council of the Institution will select for award the one whom they consider, from the particulars furnished by his college, of his career and attainments, to be the most deserving.

#### ANNUAL CONVERSAZIONE.

The President's Conversazione, which was held on July 6th, in the galleries of the Royal Institute of Painters in Water Colours, was very largely attended.

#### ANNUAL DINNER.

The third Annual Dinner of the Institution, which took place on November the 13th, was also well attended.

#### FINANCIAL POSITION.

The position in a financial point of view is satisfactory. It is true that the ordinary annual expenditure is necessarily increasing,

and that during the present year it has been further augmented by the removal of the Institution into the new offices, as already alluded to, and by the increased rent, &c., now paid; it has also been found necessary to make some addition to the Secretary's staff, but the increase in revenue arising from the large number of new members will be doubtless found to cover these items.

India  $3\frac{1}{2}$  per cent. stock to the value of £800 has been purchased on account of the "General Investment Account" out of cash standing on deposit, and £284 has also been invested in the same stock on account of Life Compositions.

## THE LIBRARY.

### REPORT OF THE SECRETARY.

I beg to report that the accessions to the Library during the year number 72, nearly all of which have been presentations from authors or publishers.

The specifications of all electrical patents continue to be supplied to the Institution, by the kindness of H.M. Commissioners of Patents. The number of patents applied for this year, up to the 30th of November, was 20,883, of which 1,337, or 6·4 per cent., were electrical. The corresponding numbers last year were 19,507 and 1,082, or 5·57 per cent.

The number of periodicals and printed proceedings of other Societies received regularly is about the same as last year, as shown by the list appended hereto.

The number of visitors to the Library during the year has been 347, of whom 25 were non-members. The corresponding numbers last year were 350 and 44 respectively, but it must be borne in mind that the Library was this year closed for over a month during the removal from the Sanctuary.

I gladly take this opportunity of reporting that Sir David Salomons has presented to the Institution a caligraph type-writer, which has proved of great assistance, and also two sets of Wheatstone A B C telegraph instruments.

F. H. WEBB,

*Secretary and Librarian.*

*APPENDIX TO SECRETARY'S REPORT.*

## TRANSACTIONS, PROCEEDINGS, &amp;c., RECEIVED BY THE SOCIETY.

**ENGLISH.**

Asiatic Society of Bengal, Journal and Proceedings.  
 Cambridge Philosophical Society, Proceedings.  
 Greenwich Magnetical and Meteorological Observations.  
 Institute of Patent Agents, Transactions.  
 Institution of Civil Engineers, Proceedings.  
 Institution of Mechanical Engineers, Proceedings.  
 Iron and Steel Institute, Proceedings.  
 Liverpool Engineering Society, Proceedings.  
 Physical Society, Proceedings.  
 Royal Dublin Society, Transactions and Proceedings.  
 Royal Engineers' Institute, Proceedings.  
 Royal Institution, Proceedings.  
 Royal Meteorological Society, Proceedings.  
 \*Royal Society, Philosophical Transactions of.  
 Royal United Service Institution, Proceedings.  
 Society of Arts, Journal.  
 Society of Chemical Industry, Journal.  
 Society of Engineers, Proceedings.  
 University College Calendar.

**AMERICAN.**

American Academy of Science and Arts, Proceedings.  
 American Institute of Electrical Engineers, Transactions.  
 Canadian Society of Civil Engineers, Transactions  
 Franklin Institute, Journal of.  
 John Hopkins University Circulars.  
 Library Bulletin of Cornell University.  
 Ordnance Department of the United States, Notes.

**FRENCH.**

Bulletin de l'Association des Ingénieurs Électriciens sortis de l'Institut  
 Electro-Technique Montefiore.  
 L'Académie des Sciences, Comptes Rendus Hebdomadaires des Séances de.  
 Société Belge d'Électriciens, Bulletin de la.  
 Société Française de Physique, Séances de la.  
 Société des Ingénieurs Civils, Mémoires.  
 Société Internationale des Électriciens, Bulletin de la.  
 Société Scientifique Industrielle de Marseille, Bulletin de la.

## LIST OF PERIODICALS RECEIVED BY THE SOCIETY.

**ENGLISH.**

Electrical Engineer.  
 Electrical Plant.  
 Electrician.  
 Engineer.

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\* Presented by Professor D. E. Hughes, F.R.S. (Past-President).

Engineering.  
English Mechanic and World of Science.  
Illustrated Official Journal, Patents.  
Industries.  
Lightning.  
Nature.  
Philosophical Magazine.  
Telegraphic Journal and Electrical Review.

#### **AMERICAN.**

Electrical Engineer.  
Electrical Review.  
Electrical World.  
Electricity.  
Journal of the Telegraph.  
Science.  
Scientific American.

#### **FRENCH.**

Annales Télégraphiques.  
L'Électricité.  
Journal de Physique.  
Journal Télégraphique.  
La Lumière Électrique.  
L'Électricien.  
Revue Internationale de l'Électricité et de ses Applications.

#### **GERMAN.**

Annalen der Physik und Chemie.  
Beiblätter zu den Annalen der Physik und Chemie.  
Electrotechnischer Anzeiger.  
Electrotechnische Zeitschrift.  
Verhandlungen des Vereins zur Beförderung des Gewerbflusses.  
Zeitschrift für Elektrotechnik.  
Zeitschrift für Instrumentkunde.

The PRESIDENT: I think we are to be congratulated upon the generally satisfactory character of the Report which your Council have been able to lay before you. We have a very considerable increase of members. I may say that, with the exception of a few of the early years, about which I do not exactly recollect how the members came in, this is by far the largest number of members we have ever had elected in one year. Increase of appreciation of this Institution seems to be going on like a rolling snowball, and what our register will be in a few years time I do not know. Even this large room will then scarcely be large enough for our meetings. The papers we have had have been very excellent: there has been, it is true, nothing strikingly

novel in them, but they have all been of a very useful and instructive character; and I think you will agree with me that the annual premiums have been awarded with very great discrimination. I may give you a little piece of information which most of you will, I am sure, be pleased to hear—viz., that Mr. Nikola Tesla is shortly to arrive in England, and has consented to give us, some time in January, a lecture on his researches in connection with alternating currents of high frequencies. We are expecting by every mail a letter containing particulars, and you may be quite sure the Council will spare no pains and no expense to place this lecture before you in a manner worthy of the lecturer and the subject, and to have it thoroughly well experimentally illustrated. Gentlemen, I beg to move the adoption of the Report as just now read.

Mr. S. EVERSHED: Perhaps you will allow me to second the adoption of the Report, as I have one or two suggestions to make as to the way in which we ballot for new members. It is rather unusual, I know, for an Associate Member to intervene in these rather formal proceedings, but on this occasion I think I may depart from the ordinary rule. One of the few rights we all, Associate and Corporate Members, exercise in common is the right of voting for new candidates. I think perhaps some of us are not sufficiently alive to the importance and necessity for constant vigilance in this matter. We all know how the voting has taken place in the past, and no doubt we are all pleased at the change just introduced. At one meeting the names of the candidates were read out, and then the list is "suspended," whatever that may mean. Then perhaps at the next meeting one or two conscientious members read the qualifications of the candidates and voted for them, and presently the ballot boxes were examined and the candidates all elected. In short, the balloting had become a mere matter of form. One good reason for the change, no doubt, is that a record should be kept. Under the old system it would have been difficult to reject a candidate if it were necessary: of course it very rarely is necessary to do so. The Institution of Civil Engineers formerly adopted the same method, but have for some time abandoned it in favour of ballot lists. Although I

have a special reason for regretting that the change was not made earlier, I would congratulate the Council on making it now; and as the Institution is growing, it is most essential that those matters should be attended to. What I wanted to suggest was a few things with regard to the new system of balloting. Under the old system one could vote for members one knew something about, and leave the others alone; under the new system we are obliged either not to vote at all or to vote for the whole of the candidates whether we know anything about them or not. Of course it ensures every candidate having some votes: perhaps that is a good thing; but it leaves the thing a bit of a farce. Is it not possible by some other way of drawing up the list a change might be made, so that we may vote either for or against a man, or not at all? Another point which struck me at the last meeting as being rather awkward was that two scrutineers had to be appointed at the meeting to examine the ballot boxes. I would suggest whether it is not possible to appoint scrutineers to act after the meeting. I noticed last time considerable reluctance on the part of any gentleman to act as scrutineer, and I have no doubt most of us were anxious to hear the discussion on Captain Sankey's and Mr. Andersen's paper. I hope the Council will give these points their consideration, and I conclude by urging on the members present the great importance of keeping up the right, and in fact the actual duty, of voting for new candidates. Of course we are all in the habit of trusting implicitly to the Council, and no doubt they are generally to be trusted. We must not judge of the zeal of the Council by the number of members we see come through the curtains; as a matter of fact, the Council meetings are very well attended indeed. But occasions may arise when some candidate is allowed to slip through the fingers of the Council, and some member may know something about that man: there may be some reason why he should be objected to. It is a most important matter that we should read through the list and see that at least no one is elected who ought to be on any ground rejected. I have great pleasure in seconding the motion that the Report be adopted.

The motion was carried unanimously.

Dr. S. P. THOMPSON: It is my pleasing duty to move a vote of thanks to the President, Council, and Members of the Institution of Civil Engineers, whose hospitality we enjoy in this building. Perhaps many of you do not know how much we are indebted to that entirely satisfactory, old-established, and well-known body of engineers, the Institution of Civil Engineers of London. Indeed, I myself had forgotten it to-night when I began to grumble at the condition of the hat pegs, until I was reminded that I ought not "to look a gift horse in the mouth." Then I asked, "Do we really pay nothing? Do we really enjoy this very valuable privilege of holding our meetings here, including the firing and lighting, free, gratis, and for nothing?" And I am assured it is so, incredible as it may seem that one Institution should lend its rooms in this magnificent way to another Institution without any reward. Think what it means to a comparatively young body like ours to have at our disposal rooms of such size and so convenient, such as we could not possibly have of our own unless we very greatly enlarged the rates of our subscriptions. We owe a very large debt of gratitude indeed to the Institution of Civil Engineers, and ought not to hesitate to express it in the most thorough manner. I beg to move, therefore—"That the members of this Institution desire to offer their cordial thanks to the President, Council, and Members of the Institution of Civil Engineers for their continued kindness and liberality in allowing the use of their hall for the meetings of this Institution."

Prof. JOHN PERRY seconded the resolution, which was carried unanimously.

Sir DAVID SALOMONS: I have been invited to perform the agreeable duty of moving this resolution—"That the thanks of the Institution are due to the Local Honorary Secretaries and Treasurers for their kind services during the past year." Before moving the resolution, I should like to answer a kind of question, if I may so call it, put by Mr. Evershed in seconding the adoption of the Report. I may say, in answer to this question, for the benefit of the other gentlemen present, that the ballot boxes were too few for the number of members now coming into the Institution. That was one reason why the ballot boxes were

given up. The reason why the new method was not adopted sooner was the fact of its entailing additional expense. But the increased number of members gives us larger funds, and enables us to adopt the new method. Speaking to the resolution, I may say that you can hardly appreciate, unless actually at the Council table, the great services the Local Honorary Secretaries render us. They are distributed all over the world. This is not an Institution, like a great many, in the sense of being a London Institution only: it is a universal one, and has its members in every quarter of the globe. The gentlemen who kindly act as our Local Honorary Secretaries look after our interests there, and bring all the flies they can into our net, collect and remit their subscriptions, and act in many other ways as means of communication between our members abroad and our Secretary in London. The least we can do is to give them a vote of thanks, and express our hope that they will continue to render as effectively the same services in the time to come as they so kindly have in the past.

The resolution, having been seconded, was carried unanimously.

Mr. A. SIEMENS: I have very much pleasure in asking you to pass a vote of thanks to the gentleman who really makes us move, who has got our sinews of war in his keeping, and without whom we cannot do anything outside this Institution. I beg to move—"That the thanks of this Institution are due "to Edward Graves, Esq., Past-President, for his continued "services as Honorary Treasurer, and for his attention to the "financial position of the Institution." We who are Members of the Council are, I dare say, better aware than you are, how very much time and energy Mr. Graves has to put to his task, and we must very much regret that illness at home prevents his being with us to-night.

The resolution, being seconded, was carried unanimously.

Professor HUGHES: I have a resolution to move, which very properly follows the last one. It is of little use our having finances if they are not properly audited and looked after carefully. Our receipts and our expenditure are increasing, and we have a great many separate funds, and the confidence we have in the correctness of the balance-sheets annually laid before

us is due to our knowledge that our accounts are well audited. I am sure you will all be ready to support me when I move—"That the thanks of the Institution are due to F. C. Danvers, Esq., and Augustus Stroh, Esq., for their kind services as "Honorary Auditors."

The resolution, being seconded, was unanimously agreed to.

Sir JAMES DOUGLASS: Mr. President and gentlemen,—This Institution finds itself in one respect in about the same position as we all do; that is, we cannot get on without the lawyers. We are very fortunate in having a very eminent firm who most kindly and liberally attend to our legal business in an honorary capacity. Probably in some years they may be let off very lightly; but this year, I am told, has been a rather heavy one. Therefore it is the more necessary to pass a vote of thanks to them. I accordingly have much pleasure in moving—"That the thanks of the Institution are due to Messrs. Wilson, Bristows, & Carpmael for the kind and valuable services "rendered by them as Honorary Solicitors."

The motion, being seconded, was carried unanimously.

Mr. G. L. BRISTOW: As I am present to-night, I would not like to allow this opportunity to pass without recording my grateful acknowledgment of the vote you have just kindly passed. I am afraid my services are very much over-estimated, for the duties I perform are mostly confined to a very pleasant interview with your Secretary, who has generally, in a very careful and judicious manner, worked out the whole question which he has had to bring before me, so that really I have had very little trouble. I am extremely obliged to you for the expression of your thanks. While I am on my legs, I would just like to refer to one other subject. Mr. Latimer Clark at the last meeting kindly introduced my name in connection with those valuable letters of the late Sir William Fothergill Cooke which he presented on the occasion. I should like to suggest that perhaps some few of us would be glad to subscribe a small fund for the purpose of printing those letters, so as to have them in a permanent form, and so that there would be no danger of their absolute loss. At present, the copy is kept at the same place

as the originals, and both might be destroyed. If they were printed, probably without expense to the Institution, copies, of course, being deposited at the British Museum and elsewhere, there would always be a permanent record of these extremely interesting and historical memoirs. Perhaps the proper way would be for me to address a letter to the Secretary, embodying this suggestion, to place before the Council.

The PRESIDENT: I am extremely glad to hear the suggestion which has been made by Mr. Bristow, and I am quite sure sufficient funds will be forthcoming to enable us to have those valuable letters printed. I will now ask Mr. Preece to read his paper.

#### ON THE SPECIFICATION OF INSULATED CONDUCTORS FOR ELECTRIC LIGHTING AND OTHER PURPOSES.

By W. H. PREECE, F.R.S., Past-President.

The deficiency in the present mode of specifying insulated conductors for electric light purposes was brought forcibly to my attention some months ago by Mr. Henry Edmunds, the London partner of Messrs. Walter Glover & Co., of Manchester; and a very able article in the *Electrician* a week or two ago brought the matter to a head, and determined me to go into the subject, and to bring it before the Institution on the earliest possible occasion. I may say at once that I have been met, in bringing this matter before you, with the greatest possible kindness on the part of every single manufacturer in this country whom I have approached. Not one man has refused to give me any information I asked for, and they have all cheerfully come forward and volunteered a great deal more than I wanted.

I almost wish that to the resolution of thanks which was voted just now to the Institution of Civil Engineers a rider had been added, which I am sure two members of their Council—Sir James Douglass and myself—would cheerfully have supported, to the effect that the value of the kind accommodation we already have here would be greatly enhanced if that accommodation included the electric light. I have the greatest possible difficulty in seeing the paper before me. It may be owing to the

deficiency of our friend up there [*pointing to the gas*], or to the absence of the electric light, to which I am now so much accustomed. Anyhow, if I do make any mistake, I beg to apologise at once in anticipation.

It cannot be said that we have hitherto been happy in our mode of specifying the insulating properties of electric light cables and conductors. Standards of quality and modes of classification differ with every manufacturer. The practice of the submarine cable engineer has been continued by the electric light engineer, and cables are specified to give so many megohms per mile, irrespective of their dimensions or of the purpose to which they are to be applied, while efforts are rarely, if ever, made to verify the figures so prominently given in published tables. If tests were made, it would be found that if the results were true for one sized conductor, they would be absolutely untrue for another. In a submarine cable, or in insulated wires for underground work, telegraphic or telephonic, the size of the conductor and the thickness of insulating coating remains constant, so that it is perfectly permissible to specify that the insulation resistance shall be  $x$  megohms per mile; but in electric light leads the diameters vary in every part of a building: a continuous mile length is rarely or never used except in mains and feeders, and the same mode of specifying is meaningless. Moreover, it is wrong, for if a conductor of 1 mm. diameter gave an insulation resistance of 2,000 megohms per mile when covered with an insulator 1 mm. thick, a conductor of 10 mm. diameter covered to the same thickness would give only 332 megohms per mile, yet its effectiveness would be the same; while, if it were constructed to give uniformly 2,000 megohms per mile, it would be absurdly constructed and wastefully paid for.

In designing an electric light lead we have to regard the insulating quality of the material to be used, and the potential differences it has to resist. Its mechanical merits we will neglect for the present. The quality of the material can be defined without reference to its form, and if it be, as it ought to be, of a uniform and consistent material, we want to know simply its

*specific insulation*, or its resistance to the passage of currents through it when subjected to potential differences.

### SPECIFIC RESISTANCE.

The specific resistance ( $\rho$ ) of conductors is thoroughly well known. The standard of reference is the resistance of a centimetre cube of some imaginary metal whose resistance is one C.G.S. unit of resistance at  $0^\circ \text{C}$ . 1,000,000,000 ( $10^9$ ) cm. of this imaginary metal 1 cm.<sup>2</sup> section give an ohm.

The following table shows the specific resistance of various metals, together with their temperature coefficients:—

		Specific Resistance.	Temperature Coefficient per $1^\circ \text{C}$ .
Silver	... ..	1,488	·00377
Copper (soft)	... ..	1,580	·00388
„ (hard)	... ..	1,616	·00388
Gold	... ..	2,036	·00388
Aluminium	... ..	2,881	·0039
Zinc	... ..	5,566	·00365
Platinum	... ..	8,957	·0034
Iron	... ..	9,611	·0048
Nickel	... ..	12,320	—
Tin	... ..	13,070	·00365
Lead	... ..	19,420	·00387
German silver	... ..	20,710	·00044
Platinum-silver	... ..	24,120	·00031
Platinoid	... ..	32,907	·00022
Nickel-steel	... ..	78,080	·00093
Mercury	... ..	94,070	·00086

Divide  $10^9$  cm. by  $\rho$ , and we obtain in any metal the length in cm. of 1 cm.<sup>2</sup> sectional area giving 1 ohm. Thus,

$$\frac{10^9}{1,580} = 632,911 \text{ cm. of soft copper of that section, and this divided}$$

by 30·48 = 20,764·8 feet, or 6,921·6 yards, which give *one ohm*.

Again,  $\frac{10^9}{94,070} = 10,630$  cm. of mercury of 1 cm.<sup>2</sup> section, or 106·3 cm. of 1 mm.<sup>2</sup> give 1 ohm.

This table also implies that if we take a length of  $10^9$  cm. of

any metal of 1 cm.<sup>2</sup> section the number ( $\rho$ ) indicates the resistance of that length in ohms. Thus, 10<sup>9</sup> cm. of soft copper of that section gives 1,580 ohms.

Conductivity in submarine cables (of the Post Office standard type) is specified as follows:—"Each conductor shall be formed " of a strand of seven copper wires, all of equal diameter, shall " weigh 107 lbs. per nautical mile, and shall at a temperature of " 75° F. have a resistance not higher than 11.65 ohms or lower " than 11.18 ohms per nautical mile."

The limits of resistance are defined in order to maintain the weight of the copper of the coils within a proper margin, and to secure the proper proportion between the metal and the dielectric. Improvement has recently taken place in the purity of the copper manufactured: the density is greater, and as a result we obtain coils which with our present mode of testing give a better result than that of Matthiessen's standard of pure copper. Coils giving 101 per cent. of pure copper are frequent, and 102 per cent. not uncommon. The manufacturers are prepared to supply pure copper; we therefore intend to abolish the percentage clause, and to abandon specifying anything but pure copper. But we must determine its specific gravity, for it appears clear that the high conductivity is due to greater density, and there is no reason to doubt the accuracy of Matthiessen's determination of the specific resistance of pure copper at a density of 8.90.

#### SPECIFIC INSULATION.

The specific resistance of insulating materials is not so well known as that of conductors, and is very variable. Clark and Sabine (1871) adopted as a standard the resistance of a cube-knot of the insulator at 75° F. (24.2° C.). But this standard never came into use, though it was tacitly admitted in all calculations derived from cylindrical cores, using the knot as unit length, and it was confined solely to submarine cables. The *knot* itself is an improper term, for a knot is a velocity, and not a length. The proper term is a nautical mile (2,029 yards) abbreviated into *naut.* Now a knot

is a "naut" per hour, and is a term confined to a class. Hence its use in any form as a general standard is out of the question. A cube-kilometre, or a cube-mile, would be a more convenient unit, but a cube-quadrant ( $10^9$  cm.)<sup>3</sup> is in harmony with our C.G.S. system of units now universally accepted. The *specific insulation* ( $\sigma$ ) of any insulating material is given by the formula,

$$\sigma = \frac{R' l 2 \pi}{\log_e \frac{D}{d}} \quad \dots \quad \dots \quad \dots \quad (1)$$

where  $R'$  is the resistance in C.G.S. units of the dielectric of a cable whose length is  $l$  cms., and whose insulating sheathing has inner and outer diameter of  $d$  and  $D$  respectively ( $D$  and  $d$  being in any units). The specific resistance in the C.G.S. system being that of a cubic centimetre of the material, the dimension of such a unit gives an excessive numerical value: it reads as high as  $10^{25}$ , C.G.S. units—a figure beyond our comprehension. A more practical dimension is the resistance of a cube whose side is 1,000,000,000 ( $10^9$ ) centimetres, taken in megohms.

Since in this country it is usual (except in the case of submarine cables) to express insulation in megohms per statute mile, the specific insulation determined from this value will be given by the expression,

$$\sigma = \frac{R \times .9144 \times \frac{1,760}{10,000,000} \times 2 \pi}{\log_e \frac{D}{d}};$$

or, giving  $2 \pi$  its numerical value and substituting common for natural logarithms, we get the expression,

$$\sigma = \frac{R \times 4.39}{\log \frac{D}{d}} \div 10,000^* \quad \dots \quad \dots \quad (2)$$

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\* A simple approximation to this formula has been worked out by Mr. H. R. Kempe as follows:—

$$\text{Log} \frac{D}{d} = 2 \left\{ \frac{D-d}{D+d} + \frac{1}{3} \left( \frac{D-d}{D+d} \right)^3 + \frac{1}{5} \left( \frac{D-d}{D+d} \right)^5 + \dots \right.$$

Now, if  $D$  and  $d$  do not differ largely, we may neglect all the terms after the first without considerable error; so that the expression,

$$\frac{R' / 2 \pi}{\log_e \frac{D}{d}}$$

where  $R$  is the insulation resistance in megohms per statute mile.

Formula (1) enables us to calculate the *specific insulation* ( $\sigma$ ) from any known dimensions when we obtain the *absolute insulation resistance*,  $R'$ .

Formula (2) enables us to obtain  $\sigma$  from the measured insulation per mile, or, *vice versa*, the insulation per mile from  $\sigma$ .

The practical standard, then, of specific insulation which I propose to adopt is the resistance of an earth-quadrant (1,000,000,000 centimetres) cube of a certain insulating material which at  $0^\circ$  C. gives a resistance of 1 megohm.

In this sense the specific insulations ( $\sigma$ ) of various materials,

becomes

$$\frac{R' l \pi}{D - d};$$

or, substituting thickness,  $t$ , of material, we get,

$$R' l \pi \cdot \frac{d + t}{t}.$$

Inserting the numerical value of  $\pi$ , and making  $R$  the resistance per statute mile, we have,

$$\sigma = R \times 5.0559 \times \frac{d + t}{t} \div 10,000,$$

which may be written approximately (especially since we know that  $\frac{d + t}{t}$  is actually rather too large).

$$\sigma = R \times 5.05 \times \frac{d + t}{t} \div 10,000.$$

For general purposes the expression.

$$R \times 5 \times \frac{d + t}{t} \div 10,000,$$

or

$$R \times \frac{d + t}{\frac{t}{2,000}},$$

will give sufficiently correct results, being within 1 per cent. of accuracy in most cases. Thus, taking  $D = .5$ ,  $d = .314$ ,  $R = 5,000$  (the actual results obtained with a rubber insulated wire), then  $t = .093$ : by the exact formula we have,

$$\begin{aligned} \sigma &= \frac{5,000 \times 4.39}{\log .5 - \log .314} \div 10,000 \\ &= \frac{5,000 \times 4.39}{1.6989700 - 1.4969296} \div 10,000 = 10.865. \end{aligned}$$

By the approximate formula we get,

$$\frac{5,000 \times \frac{.314 + .093}{.093}}{2,000} = 10.941.$$

calculated from dimensions and resistances, but not corrected for temperature, are as follows:—

Material.	C.G.S. Units.	Proposed Standard.	Temperature.	Authority.
Air ... ..	$\infty$	$\infty$		
Mica ... ..	$8.4 \times 10^{22}$	0.084	20° C.	Ayrton & Perry.
Gutta-percha	$4.5 \times 10^{23}$	0.45	24° „	L. Clark.
India-rubber	$1.09 \times 10^{23}$	10.9	24° „	Jenkin.
Shellac ... ..	$9.0 \times 10^{24}$	9.0	28° „	Ayrton & Perry.
Hoopers' core	$1.5 \times 10^{25}$	15.0	24° „	Tests.
Ebonite ... ..	$2.8 \times 10^{25}$	28.0	46° „	Ayrton & Perry.
Paraffine ... ..	$3.4 \times 10^{25}$	34.0	46° „	„ „
Glass (Flint)...	$2 \times 10^{25}$	20.0	20° „	T. Gray.
„ (ordinary)	$9.1 \times 10^{22}$	0.091	20° „	Foussereau.
Paper (parchment)	$3 \times 10^{20}$	0.0003	20° „	Uppenborn.
„ (ordinary)	$4.85 \times 10^{18}$	0.000000485	20° „	„
Siemens's special high insula- ting rubber... ..	...	16.17	15° „	Siemens.
Siemens's ordinary pure and vulcanised rubber...	...	2.28	15° „	„
Siemens's specially high in- sulating fibrous material	...	11.90	15° „	„
Fowler-Waring dielectric ...	...	7.33	15° „	Tests.
*Vulcanised rubber ... ..	...	1.5	15° „	„

To obtain the true value for  $\sigma$  these results, of course, ought to be corrected to 0° C.

### ELECTRIFICATION.

Insulated conductors should always be subjected to the process of electrification for a certain period before the scale reading from which the insulation is to be calculated is noted. The leakage current polarises the dielectric; the insulation apparently improves—rapidly at first, then more slowly—and unless some conventional time were agreed upon for the test, proper com-

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\* Vulcanised rubber varies very much in its specific insulation, for it depends so much on the compound used, and on the mode of curing. The figure is a mean of several samples of different makers.

parisons could not be made. One minute is universally accepted as the period. The rate of fall due to electrification is a great test of the quality of the insulating material. It varies with the nature and quality of the material, being more marked at low temperatures. Unsteady electrification is a sign of an incipient fault, and perfectly regular electrification is an indication of good material. The resistance of the dielectric apparently gradually increases, owing to the formation of an opposing electro-motive force in what is probably a liquid electrolyte. As the rate at which electrification proceeds is an uncertain quantity, and in the best material is very low, it is better to specify that the electrification shall progress steadily, without stating the rate at which it proceeds. The temperature at which the test is to be made should also, of course, be given, 75° F. being the usual standard.

#### THICKNESS OF INSULATION.

More important is it to determine and specify the thickness of the insulating coating. Two conditions determine this—the E.M.F. to be used, and the mechanical conditions of manufacture. We have also to consider sparking distances in air, especially in the case of high-pressure mains, when heat, age, accidents, and use cause decay and cracks, and the conductor becomes exposed through fissures. I cannot contemplate a less thickness of any material than 1 mm., or .04 of an inch, for a No. 19 wire. That means that a millimetre conductor requires as a minimum a millimetre thickness of insulating coating. Now the striking pressure of a millimetre in air is over 600 volts. As the conductor increases in diameter the thickness must increase in greater ratio, not because the E.M.F. requires it, but because the exigencies of manufacture and the mechanical construction of the core demand more stuff to resist the stresses brought to bear upon it. Given a conductor with a certain thickness of dielectric able to resist the stress of  $x$  volts, the same thickness will equally resist the same voltage whatever the diameter or sectional area of the conductor.

The actual thickness to be used above the crucial thickness required for the security of those who accidentally or purposely handle the wire is a manufacturer's question; the engineer can

specify only the minimum thickness. The Board of Trade rule for aerial conductors is the following:—

“Every high-pressure aerial conductor shall be continuously insulated with a durable and efficient material, to be approved by the Board of Trade, to a thickness of not less than one-tenth part of an inch; and in cases where the extreme difference of potential in the circuit exceeds 2,000 volts, the thickness of insulation shall not be less in inches or parts of an inch than the number obtained by dividing the number expressing the volts by 20,000. This insulation shall be further efficiently protected on the outside against injury or removal by abrasion. If this protection be wholly or partly metallic, it shall be efficiently connected to earth.

“The material used for insulating any high-pressure aerial conductor shall be such as will not be liable to injurious change of physical structure or condition when exposed to any temperature between the limits of 0° and 150° Fah., or to contract with the ordinary atmosphere of towns or manufacturing districts.”

The two points, then, to be met are those of specific insulation and of thickness; the former determining the quality of the material, and the latter being governed primarily by the striking distance.

It is an extremely difficult thing to arrive at a definite conclusion as to the exact sparking distance across an air space with a given potential difference. It varies with the character of the opposing surfaces; with the resistance, capacity, and electromagnetic inertia in the circuit; with the nature of the current—that is, whether it is steady or alternating, and also in the latter case with the frequency. Sir W. Thomson concludes “that a Daniell’s battery of 5,510 elements can produce a spark between two slightly convex metallic surfaces at one-eighth of a centimetre asunder in ordinary atmospheric air” (“Papers on Electrostatics and Magnetism,” page 259), which means that it requires 4,928 volts to strike across one millimetre.

Warren de la Rue obtained a result very similar. Professor Crookes, using an induction coil, takes much lower figures, and

estimates the striking E.M.F. at 1 mm. as 920 volts. Mr. Ferranti, using a frequency of 200, and 20,000 volts, brought the voltage down to 620.

I propose for underground and covered conductors to specify as a minimum for 500 volts or any E.M.F. under, 1 mm. thickness of dielectric, and .5 mm. for each succeeding 500 volts or portion of 500 volts.

*Thickness of Dielectric for different Voltages.*

Voltage.	Mm.	Inch.	Board of Trade.
500 and under	1.0	.04	.04
1,000 ... ..	1.5	.06	.05
1,500 ... ..	2.0	.08	.075
2,000 ... ..	2.5	.10	.10
2,500 ... ..	3.0	.12	.125
5,000 ... ..	5.5	.22	.25
10,000 ... ..	10.5	.42	.5

CLASSIFICATION.

The classification of insulated conductors is most varied and perplexing as regards the dielectric. The most systematic method is that of Siemens as regards the conductor. The pattern number gives the length in yards which has a resistance of one-tenth of an ohm; divided by 4, it gives its carrying capacity in amperes; divided by 5, it gives its weight in lbs. per mile; divided by 6, it gives its sectional area in mm.<sup>2</sup>; divided by 4,000, it gives its sectional area in square inches. Nothing can be more charming. But when we come to insulation we have types G, H, J, P, Q, R, L, M, N, LL, MM, NN, LLL, MMM, and NNN—all differing in the number and thickness of layers of pure rubber, of vulcanised rubber, of "special prepared material," in taping, braiding, preserving with compound, lead, tarred jute yarn, and iron sheathing, and also in insulation resistance per statute mile in megohms at 60°.

N, NN, and NNN are intended for 250, 2,500, and 5,000 volts respectively, and they vary in price in the ratio 64, 79, 93.

We are, however, told that they are tested in water *when not intended for dry places*, and Messrs. Siemens show that the

insulation resistance diminishes as the size of the conductor increases.

For instance, type R: "Insulated with one layer of pure and "two of vulcanised rubber, taped [braided, and drawn through "preservative compound]. Tested under water, insulation resistance from 2,000 to 700 megohms per statute mile at 60° F., "according to size of wire." The words in brackets form the sole difference between types J and R!

The Silvertown Co. work up by areas of conductor, advancing by twentieths of a square inch, between 1-10 square inch and 1 square inch. The insulator is composed of pure rubber—their best vulcanised india-rubber. They used to endeavour to maintain a constant ratio of diameters of conductor and dielectric, but the insulation resistance now varies with size. The wires are all rigidly tested under water, and give over 2,500 megohms per mile.

But the classification into letters is as perplexing as with nearly all the other makers, except, perhaps, the Fowler-Waring Co., who have only one dielectric and one form of protection, viz., lead covering.

The Standard Underground Cable Co. of U.S.A., who use Waring's compound, specify the thickness of insulation in mils. Their smallest is 31 mils, 9 mils less than the proposed minimum. Their thickest is 188 mils, or 7.5 mm., which is good enough for 7,000 volts.

This classification into types is a manufacturer's question. We cannot interfere with it, but we do earnestly hope that they will study the convenience of the user by reducing their types to the lowest possible number, but, above all, by removing from their lists the cheap and nasty.

The evil of this loose system was strikingly exemplified in a case which recently came before the law courts, when some of the ablest counsel were busily employed for several hours in endeavouring to explain what was meant by "C" quality wire, with but little result, none of the expert witnesses present being able to throw any light on the subject.

I am much indebted to different firms for the information

which they have cheerfully given me to enable me to determine the specific insulation of the different materials they use; and as this information may be very useful, I tabulate it, and submit it herewith.

This table must not be taken as a competitive examination of the relative merits of the different goods. I have in every case taken them just as they came, and I have not indicated the classification, simply because I did not know it.

*Specific Insulation ( $\sigma$ ) of Electric Light Cables manufactured by various Firms.*

Manufacturer.	CONDUCTOR.				DIELECTRIC.				Insulation per Mile at 60° F.: Megohms.	Specific Insulation ( $\sigma$ ).	Remarks.
	No. of Wires and approximate gauge.	Diameter (d).		Thickness (t).		Diameter (D).					
		in.	mm.	in.	mm.	in.	mm.				
Siemens Bros. }	7/22	·084	2·14	·076	1·93	·236	5·99	16,180	15·84	} "Siemens's special" high insulating india-rubber.	
"	"	"	"	·078	1·98	·240	6·10	18,270	17·60		
"	19/15½	·340	8·64	·080	2·03	·501	12·72	1,022	2·67	} Ordinary pure and vulcanised rubber.	
"	61/15½	·612	15·54	·168	4·27	·949	24·10	824	1·90		
"	51/12	·850	21·59	·190	4·82	1·230	31·24	4,082	11·17	} Lead-cased; insulated with specially high insulating fibrous material. (Bradford Corporation mains.)	
"	37/13	·651	16·53	·096	2·44	·844	21·43	2,567	10·00		
"	19/14	·410	10·41	·079	2·01	·568	14·42	3,794	11·77		
"	7/13	·286	7·26	·079	"	·444	11·28	6,389	14·69		
"	60/6½	1·656	42·06	·113	2·87	1·882	47·80	624	4·93	} Lead-cased; insulated with impregnated fibrous material. (St. James's and Pall Mall Co.'s mains.)	
"	"	"	"	"	"	"	"	830	6·56		
"	60/10	1·170	29·72	·114	2·89	1·398	35·51	851	4·83		
"	"	"	"	"	"	"	"	1,014	5·76		
Silvertown	19/15	·360	9·14	·144	3·66	·648	16·46	6,768	11·64	} Class L.—Special pure india-rubber, then vulcanised india-rubber.	
"	"	"	"	"	"	"	"	6,664	11·46		
"	"	"	"	"	"	"	"	6,549	11·26		
"	19/16	·320	8·13	·80	2·03	·480	12·19	592	1·48	} Class J.—Pure india-rubber, then vulcanised india-rubber.	
"	19/18	·240	6·10	·65	1·65	·369	9·37	585	1·37		
"	"	"	"	"	"	"	"	526	1·24		
"	7/16	·192	4·88	·56	1·42	·305	7·75	607	1·33		
"	19/18	·240	6·10	·65	1·65	·369	9·37	910	2·21	} Class K.—Pure india-rubber, then vulcanised india-rubber.	
"	"	"	"	"	"	"	"	1,097	2·58		
"	"	"	"	"	"	"	"	1,097	2·58		
"	7/16	·192	4·88	·56	1·42	·305	7·75	1,215	2·65		
Callender	7/16	·192	4·88	·154	3·91	·500	12·70	380	·40	} Vulcanised bitumen.	
"	19/18	·240	6·10	·155	3·94	·550	13·97	400	·49		

*Specific Insulation ( $\sigma$ ) of Electric Light Cables manufactured by various Firms—(continued).*

Manufacturer.	CONDUCTOR.			DIELECTRIC.				Insulation per Mile at 60° F.: Megohms.	Specific Insulation ( $\sigma$ ).	Remarks.
	No. of Wires and approximate Gauge.	Diameter. (d).		Thickness (t).		Diameter (D).				
		in.	mm.	in.	mm.	in.	mm.			
Glover	19/13	·460	11·68	·115	2·92	·690	17·52	600	1·50	} Ordinary pure and vulcanised rubber. In use at the G.P.O. (Specified 300 megohms per mile.)
"	"	"	"	"	"	"	"	515	1·28	
"	"	"	"	"	"	"	"	560	1·40	
"	7/16	·192	4·88	·055	1·40	·302	7·67	855	1·91	
"	"	"	"	"	"	"	"	813	1·81	
"	"	"	"	"	"	"	"	833	1·86	} Waring compound.
Fowler-Waring }	1/18	·048	1·22	·036	·91	·120	3·05	6,522	7·20	
"	1/16	·064	1·62	·043	1·09	·150	3·81	6,619	7·85	
"	7/16	·192	4·88	·078	2·03	·352	8·94	4,835	8·07	
"	19/18	·210	6·10	"	"	·400	10·16	3,280	6·48	
"	19/16	·320	8·13	·095	2·41	·510	12·95	3,312	7·18	} Ordinary pure and compound india-rubber.
"	19/15	·360	9·14	·100	2·54	·560	14·22	3,145	7·19	
Henley	1/10	·128	3·25	·046	1·17	·220	5·59	564	1·05	
"	7/16	·192	4·88	·054	1·37	·300	7·62	406	0·92	
"	19/18	·210	6·09	·060	1·52	·360	9·14	329	0·82	
"	1/14	·080	2·03	·040	1·02	·160	4·06	4,440	6·48	} Ozokerited india-rubber.
"	3/20	·078	1·98	·039	·99	·157	3·99	5,240	7·57	
"	7/15	·216	5·49	·057	1·45	·330	8·38	2,210	5·27	
"	19/14	·400	10·16	·080	2·03	·560	14·22	2,000	6·01	} Special cable—Class AA.
"	61/15	·648	16·46	·111	2·82	·870	22·09	2,200	7·55	
"	61/12	·936	23·77	·217	5·51	1·370	34·79	7,146	19·98	

### SPECIFICATION.

The consulting engineer can afford to smile at classification, for he can draw up his own specifications, but it is extremely difficult even to do this without a reference to some well-known maker's class.

No wire should ever be used which cannot be subjected to a test under water. Insulated wire is supplied which is specified *to be used in dry places only*. But as this stuff is cheap, inspection is ignored, and dry places are not always dry; its general use follows: dampness and leakage result, electrolysis sets

in, the conductor is eaten away, and the insulating coating is charred or even set on fire.

It is impossible to speak too strongly on the terrible abuse of ordinary precautions which is encouraged by the manufacture of these cheap cables. It destroys confidence, introduces danger, and discourages the industry.

A very curious practice has recently developed itself among our wiring contractors. They have asked manufacturers to reduce the over-all diameter of their insulated conductors in order to enable them to utilise the grooved boarding which they happen to have in stock. In other words, the interests of the users of the electric light are to be sacrificed to the interests of the wood-carvers!

I do not believe in danger to person in high-pressure systems when proper material is used. They are so safeguarded that any contact between primary and secondary—practically the only source of danger—leads at once to safety. I fear more danger of fire in both systems, and this more in low-pressure systems than in high-pressure ones, for they are not so safeguarded and are more open to abuse. In fact, the numerous accidents that have occurred from imperfect work on low-pressure systems has already attracted serious attention to its dangers.

I prefer, then, to specify that—

1. The copper must be pure, and such that at a density of 8.9 its specific resistance is 1,616; that is, it must comply with Matthiessen's standard of pure hard drawn copper.
2. If it is to be covered with any vulcanised material, it must be *tinned*.
3. No size smaller than No. 19 (.04 in. or 1 millimetre wire) is to be used.
4. No single solid wire greater than No. 16 is to be used.
5. All larger sizes are therefore to be stranded.
6. The weight of copper used is to be such that the loss of volts between the distributing switch and the furthest lamp when full load is on shall not exceed 1 per

cent. Hence the current which each conductor has to carry must be given.

7. The dielectric to be employed must be described, with its specific insulation, in terms of a quadrant-cube of gutta-percha at 75°, which is taken as unity.\*
8. The thickness of dielectric must be given for the different sized conductors used.
9. The various modes of preserving and protecting the insulated cables must be given.
10. Every coil should have attached to it the maker's certificate, testifying that it complies with this specification, and that it has been tested under water.†

#### TESTING.

There are upwards of 130,000 miles of cable submerged in the ocean, and I cannot recall to mind a single fault that has occurred since 1865 through a manufacturer's defect that has escaped the probe of the tester. Every cable is most rigidly and carefully tested under water. Why do not users do the same with their electric light conductors? It would be the greatest safeguard for security.

There is a widespread impression that the insulation varies inversely with the potential differences employed, and that the higher the voltage used the more accurate the result. This is a mistaken notion. The following experiment was made to show its fallacy:—

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\* This can easily be deduced from its insulation in megohms per mile when the dimensions and the temperature coefficient for the material are given.

† Messrs. W. Glover and the Silvertown Co. already do this.

*Table showing Results of Insulation Tests of Gutta-Percha Core with various Voltages.*

Number of Cells. (Leclanché).	Galvanometer Readings.			Insulation after 1 Minute's Electrification.
	1st Min.	2nd Min.	3rd Min.	
				Megohms.
10	18½	16	14½	164·3
40	76	67	64	162·2
100	179	163	157	173·3
200	181    7,000 ω	168	162	168·2
300	192    4,000 ω	181	177	172·2
400	188    3,000 ω	177	173	174·1

The advantage of high voltage is that it breaks down incipient faults; but, on the other hand, if too high, it may put in faults; it is therefore advisable that the insulation be tested with a voltage, say, not less than 50 per cent., and not more than 100 per cent. above the voltage it is intended to resist.

#### DURABILITY.

The question of durability is not less important than the question of insulation, and experience has proved that india-rubber, and other materials which give the highest insulation when a cable is newly manufactured, will not necessarily give satisfactory insulation after it has been in use for a comparatively short period. I have very little confidence in the durability of materials which give a specific insulation much above the normal. We have had to limit the range of gutta-percha, because extra high insulation is a certain indication of impurities and short life. It is impossible to predicate what will happen with india-rubber, for high insulation has come in only recently with extra high pressures, though I fear trade competition and manufacturing tricks have something to do with it. I am quite content with the normal specific insulation. The durability of new insulating materials can only be proved by actual trial; but

in the case of vulcanised rubber, which is more largely used than any other material, the test applied by the Post Office for ordinary rubber goods fortunately gives the information required as to durability. Everyone will have observed how inferior vulcanised rubber, such as is often used for inferior india-rubber rings, tube, and other cheap articles, will crack and become useless frequently in a few months; and unfortunately the use of inferior rubber for insulating electric light cables is more common than might generally be supposed; indeed, it is difficult to get samples from two or more makers with the rubber of equal quality in each.

Our test at the Post Office to determine the durability of rubber goods is to apply to a sample a dry heat of  $270^{\circ}$  Fah. for one hour, and a moist heat of  $320^{\circ}$  for three hours. Vulcanised rubber which will satisfactorily stand these tests under ordinary circumstances—*i.e.*, when used for tube, sheet, &c.—ought to last not less than 15 years in an electric light cable when the current is not allowed to unduly heat the wire. If, however, an excess of current is allowed to heat the conductor, the life of india-rubber must be very seriously shortened.

In this paper I have simply trodden on the fringe of the question. There are many points I have left untouched—many points which await investigation and inquiry. We want time and experiment to work out several points, especially one to which I have alluded to, *viz.*, our ignorance of the striking distance in air.

I do not propose to ask the President to allow a discussion on this paper to-night; I simply offer it to your further consideration. I may tell you this—that Mr. Alexander Siemens is already taking up one part of the subject; and when we meet again Mr. Siemens will read his paper, and then I hope we shall have a discussion on the whole question. I am afraid there are very many deficiencies in the paper, but I cannot urge too strongly on you the fact that the industry is being seriously injured by the faults I have endeavoured to describe to you.

The PRESIDENT: I propose a hearty vote of thanks to Mr.

Preece for his very excellent paper. I think there can be no doubt that the best course will be to carry out Mr. Preece's suggestion and discuss the paper after Mr. Siemens's paper has been read. Our next meeting will be held on January 14th, when Professor Ayrton will give his Inaugural Address, and it is not at all unlikely that the meeting after that will be occupied by Mr. Nikola Tesla; but as soon as we can arrange a meeting for the purpose, it will be devoted to Mr. Siemens's paper, and to the discussion of it and Mr. Preece's.

The vote of thanks to Mr. Preece was carried by acclamation.

The Scrutineers having handed in their report, the result of the ballot for new Council and Officers was announced by the President as follows :—

*President :*

Professor W. E. AYRTON, F.R.S.

*Vice-Presidents :*

ALEXANDER SIEMENS, M. Inst.  
C.E.  
R. E. CROMPTON, M. Inst. C.E.

Sir DAVID SALOMONS, Bart.,  
M.A.  
Sir HENRY MANCE, C.I.E.,  
M. Inst. C.E.

*Ordinary Members of Council :*

Major G. W. ADDISON, R.E.  
Sir ALBERT J. L. CAPPEL, K.C.I.E.  
Sir JAMES DOUGLASS, F.R.S.  
Professor J. A. FLEMING, M.A.,  
D.Sc.  
Professor GEORGE FORBES,  
F.R.SS. (L. & E.).  
Dr. EDWARD HOPKINSON, M.A.  
Colonel R. RAYNSFORD JACKSON.

Professor A. B. W. KENNEDY,  
F.R.S., M. Inst. C.E.  
Professor J. PERRY, D.Sc.,  
F.R.S.  
W. M. MORDEY.  
JAMES SWINBURNE.  
Professor SILVANUS P. THOMP-  
SON, B.A., D.Sc., F.R.A.S.

*Associate Members of Council :*

Capt. L. A. BEAUMONT, R.N. | M. COOPER.  
Captain W. C. HUSSEY, R.E.

*Honorary Auditors :*

FREDERICK C. DANVERS. | AUGUSTUS STROH.

*Honorary Treasurer :*

EDWARD GRAVES, Past-President.

*Honorary Solicitors :*

Messrs. WILSON, BRISTOWS, &amp; CARPMAEL, 1, Copthall Buildings, E.C.

The following candidates for admission into the Institution were declared to be duly elected, viz. :—

*Members :*

Edgar Bloxham.	Matthew William Walbank
Professor George Francis	Mackie.
Fitzgerald, F.R.S.	William Howard Winter-
Alec Gavin Inrig.	botham.

*Associates :*

Ernest Albert Browning.	F. Lilley.
Adrian Charles Collins.	William Macpherson.
Philip Dawson.	Francis Joseph Moffett, B.A.
Henry Bligh Forde.	Richard North.
Archibald E. Gosset-Tanner,	John Edward Preston.
B.A.	Theodore William Pritchett.
Arnold Kennard Greener.	Arthur George Savill.
Ernest Holmes.	Alfred Whalley.
Alexander Kiatibian.	Dr. Robert Lloyd Wooll-
Charles Herbert Lees.	combe, LL.D., M.R.I.A.

*Students :*

Jules Ferdinand Conradi.	Henry Payne.
Walter Harry Jackson.	John A. Russell.

Upon the motion of the PRESIDENT, the thanks of the meeting were unanimously voted to the Scrutineers for their kind services.

## ABSTRACTS.

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### **J. VANNI**—THE APPARENT VARIABILITY OF THE ELECTRO-CHEMICAL EQUIVALENT OF COPPER.

(*Wiedemann's Annalen*, Vol. 44, p. 214.)

Gray has pointed out that the quantity of copper deposited per coulomb varies to some extent with the current-density, making the equivalent vary from '0003287 with 20 milliamperes per sq. cm. to '0003278 with 3·3 milliamperes, both at 12° C.; and at 35° C. getting as low as '0003245. This variation he attributed to the redissolving of the cathode by the solution during the deposit, but did not prove it. The author has investigated the matter experimentally, and confirms Mr. Gray's assumption. Passing the current for three hours through two depositing cells in series, one with a large and the other a small cathode, he found the deposit on the former considerably less than that on the latter. Then, replacing the cathodes in their cells, he noted the loss of weight of each when standing idle for the same time; this loss of weight for each plate, when added to the previous weight of the deposit on each, made the two deposits equal, within the limits of error. In this experiment the solution contained about 1 per cent. free sulphuric acid. When the plates were left idle in a perfectly neutral solution they gained slightly in weight. By adding to a neutral solution '005 gram of acid per litre, the author obtained a solution in which the plates neither gained nor lost weight when idle, and with which exactly similar deposits were obtained with current-densities varying from 5 to 21 milliamperes per sq. cm. He considers, therefore, that Gray's explanation of the variation is satisfactorily proved.

### **Dr. O. FRÖLICH**—THE ELECTRICAL PRODUCTION OF OZONE.

(*Electrotechnische Zeitschrift*, Vol. 12, No. 26, p. 341.)

After mentioning various laboratory appliances for the purpose, the author describes the plant, as manufactured by Messrs. Siemens & Halske, for the production of ozone on a commercial scale. The apparatus in which the discharge takes place is built up of an inner metal tube, kept cool by water, and forming the inner coating; this is surrounded by a celluloid or ebonite tube, and through the annular space between these two a continuous flow of oxygen is kept up. The outer surface of the celluloid tube has a metallic sheath which acts as the outer covering, and between which and the inner metallic tube a constant silent discharge is kept up. Ten such tubes are fixed together in a frame and joined up in parallel, both electrically and with regard to gas and water supply. An open circuit transformer is used, excited by a set of accumulators which also drive a small motor, by the spindle of which a make-and-break in the primary circuit is worked. The rapidity of make-and-break should be high, say 600 per sec. The sharper the variations in the secondary circuit the greater the production of ozone,

and for a few tubes the interrupted direct current gives much sharper variations than an ordinary sine curve alternating one. When many tubes are used, however, their increased capacity affects the former and rounds its wave down to a considerable extent, while it does not appreciably affect the alternating current, which is therefore to be preferred when very many tubes are used. There appears to be a certain number of  $\sim$  which gives a maximum of ozone. The production also increases up to a certain point with the rate of the passage of gas through the tubes, but beyond this point increasing the gas has no effect.

The amount of energy expended in the actual formation of ozone is extremely small. It makes very little difference in the total work done if the space between the tubes be filled with gas, water, or highly rarefied air. A 2-H.P. apparatus produces about 2.4 milligrammes of ozone per second. It would greatly increase its usefulness if ozonised air could be compressed like oxygen, but this presents difficulties, as special pumps must be used. So far, the author has not got beyond 9 atmospheres. Various uses of ozone are mentioned, such as disinfecting ships and buildings, destroying insects, and sterilising water, treating sewage, &c., and also for brewing purposes.

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#### D. HURMUZESCU—VIBRATIONS SET UP IN A WIRE BY A CONTINUOUS CURRENT.

(*Comptes Rendus*, Vol. 113, No. 3, p. 125.)

A fine metallic wire stretched between two supports and traversed by a current will begin to vibrate with increasing amplitude till a maximum is reached, which is maintained as long as the current remains constant and the surrounding air undisturbed; when the wire is enclosed in a tube the vibrations are more regular. The vibrations vary with the current, the size of the wire, and its tension, and cease in a few seconds if the current is broken. The author attributes the phenomenon to the heating of the wire.

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#### L. HOLBORN—THE HARDENING OF MAGNET STEEL.

(*Zeitschrift für Instrumentenkunde*, Vol. 11, p. 113, 1891; *Beiblätter*, Vol. 15, No. 8.)

Samples of different steels were heated in a furnace to various temperatures and hardened in water; they were then magnetised in a coil and the temporary and the permanent magnetism determined: both were greatly affected by the hardening temperature, and the effect of differences in the hardening temperature increased with the increase of carbon in the steels. Each steel had a particular temperature, which gave a maximum permanent magnetism—silver steel, a little below 900°; tungsten steel, 920°; and a third sort, 850°, &c. When quenched at the temperature which gives greatest permanent magnetism, a magnet is no more liable to lose its magnetism from rough usage than when it has been hardened at a higher temperature.

— **RIGHI**—ON DISCHARGES CONSISTING OF MOVING LUMINOUS CLOUDS.

(*Lum. El.*, Vol. 42, No. 40, p. 38 [*Rend. Lincei*].)

These are experiments on the discharge of a powerful battery (E.M.F. not stated) through more or less rarefied air, in which case, under certain circumstances, and especially when there is a large resistance in the circuit, a reddish or rosy flame seems to stretch out from the positive electrode and enlarge and vanish before it reaches the negative one. Analysis with the revolving mirror shows this flame to consist of a number of luminous masses, or clouds, moving comparatively slowly (in one case, for example, 17 cm. in  $\frac{1}{8}$  second); or, says the author, we may equally well express it that luminous properties are communicated to successive portions of gas. The effects may be much modified by altering capacity, resistance, air pressure, diameter of tube, &c.

If the tube be small, the luminous cloud, when it reaches a certain distance from the negative electrode, will sometimes return a little towards the positive instead of dispersing at once. Numerous instantaneous photographs were taken, which showed the moving clouds to be what might be described as a hemisphere with a conical tail; bright at the centre, and nebulous towards the surface. Each cloud leaves the  $+$  electrode much as a drop of water leaves the end of a glass rod, and a little luminosity remains behind on the electrode. The author suggests that if such discharges could be produced in air at ordinary pressure and given a slower motion, we might have "globular lightning." If the tube is constricted in the middle, the constriction remains luminous during the whole discharge, and acts as an intermediate electrode; *i.e.*, the clouds from the  $+$  electrode stop before they reach it, while the opposite end of the stricture gives off clouds like the  $+$  electrode. When the circuit is rapidly made and broken, the clouds vanish at the point they reached at the instant of break, and begin again at the  $+$  electrode when the circuit is made again. If, however, instead of a dead break, a large resistance is alternately thrown in and out of circuit, the luminous cloud reappears when the resistance is cut out at approximately the same place that it vanished at when the resistance was introduced.

The effect of a magnet on the discharge resembles that of a constriction in the tube; a fixed luminous cloud acting as an intermediate electrode.

The author promises a further and fuller paper shortly.

**D. BERTHELOT**—ELECTRICAL RESISTANCE IN CONNECTION WITH THE NEUTRALISATION OF ACIDS.

(*Journal de Physique*, Vol. 10, p. 458.)

Electrolytes may be divided into two groups; the first comprises all strong acids, strong bases, and neutral salts. All these bodies are good conductors even in concentrated solutions, and their molecular conductivity varies but little with dilution. The second group consists of weak acids, such as most organic ones; and weak bases, such as ammonias. In concentrated solution they conduct badly,

but when much diluted almost as well as the first group. The combinations of strong acid and strong base, strong acid and weak base, &c., are then discussed, and curves of resistance show a more or less sudden change at the point of neutralisation.

**F. J. SMITH—EFFECTS OF MAGNETISM ON RODS OF IRON, NICKEL, &c., WHICH HAVE RECEIVED A PERMANENT TORSIONAL SET.**

(*Phil. Mag.*, Vol. 32, p. 383.)

A well-annealed iron rod 50 cm. long  $\times$  .162 cm. diameter was placed in a magnetising spiral and fixed at one end; to the other end a mirror was attached, by which twisting of the rod could be measured by telescope and scale. On passing a current through the spiral (giving a force  $H=21.5$ ), the rod twisted, returning instantly to its original position on the circuit being broken, the deflection being 1 mm. with the scale 180 cm. distant from mirror. A similar rod was then given a permanent torsional set of seven revolutions and subjected to the same magnetising force. This produced a further torsion in the *same* direction as the initial torsion of 5 mm.; on breaking the current, the rod returned to its former state; on reversing the current, the direction of torsion did *not* reverse. The amount of torsion was proportional to  $H$  for low values, then became constant, and by forcing  $H$  higher still, began to decrease. Keeping  $H$  constant, the deflection increased with increased initial torsion of the rod up to a maximum value, then decreased with increased initial torsion. With initial torsion of 10 and 15 complete turns the rod began to slowly untwist after each magnetisation—*e.g.*, initial scale reading 60, with the current on 69, current broken 59. With nickel, this step-by-step unwinding of torsion after magnetisation was very evident; the general behaviour of nickel being similar to that of iron, but less marked. Negative results were obtained with non-magnetic metals, Cu, St, Pt, Pb, Ag, Zn, Cd, brass, and bronze; nor was any effect obtained with bismuth.

When a freshly annealed iron core was placed in the spiral and the ends of the core joined to a ballistic galvanometer, a deflection of the latter was obtained when the current was passed through the spiral. On breaking the latter current, no effect was obtained on the galvanometer, nor was there any on making the magnetising circuit again, though in a later experiment a very small deflection was obtained on making and breaking after the first time. On reversing the magnetising current, the galvanometer gave a deflection equal to that first obtained, and in the opposite direction; but subsequent makes had no effect till the current was again reversed. It is only when the magnetism of the bar is entirely reversed that the transient current is produced in any marked degree. In the above the cores were free from torsion. No effect was obtained with brass or copper cores.

With iron cores having permanent torsional set the same transient current was produced on reversal of magnetism, but it was much larger in one direction than in the other; depending on the direction of the torsion.

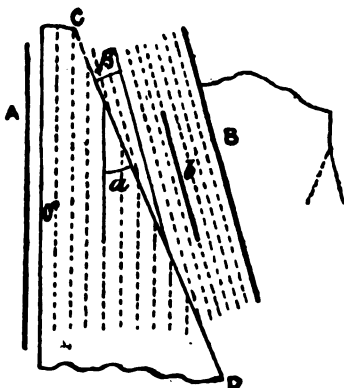
When the current in the earlier experiments was made and broken by a tuning-fork, a twisted iron core gave a loud musical note, due to torsional vibration; and if a light pointer was attached to the rod a record of the vibrations could be obtained

on a smoked glass. There was no difficulty in obtaining such a record with 1,000 vibrations per second. The "latency" of this form of stylus has been shown by repeated experiments to be very small, which makes it very useful for chronographic work.

#### A. PEROT—VERIFICATION OF THE LAW OF DEVIATION OF EQUIPOTENTIAL SURFACES, AND DETERMINATION OF S.I.C.

(*Comptes Rendus*, Vol. 112, No. 13, p. 415.)

If  $K_1$  and  $K_2$  be the S.I.C. of two media, and  $\alpha$  and  $\beta$  the angles which an equipotential surface forms with the surface of separation of the media, we have,  $\tan \alpha : \tan \beta :: K_1 : K_2$ ; or, if the second medium is air,  $\tan \alpha = K \tan \beta$ .



The author took a large prism of resin, O C D, and placed against it a metallic plate, A, which he raised to about 30,000 volts. If the plate and prism are sufficiently large, the equipotential surfaces in the resin will be, as shown by the dotted lines, parallel to D C. In the air outside they will be parallel among themselves, but at an angle to those in the prism. If a conducting plate, B, were now placed parallel to the equipotential surfaces, its potential would in no way be altered if another smaller plate,  $b$ , were moved backwards and forwards parallel to B—i.e., along another equipotential surface. By trial and error such a direction was found for B, and only one such direction; but B could then be moved nearer to or further from the prism parallel to itself without the movements of  $b$  parallel to it causing any deflection of the gold-leaf electroscope attached. It was then considered that B was in the direction of the equipotential surfaces in the air. The angles  $\alpha$  and  $\beta$  were then measured, and  $K$ , calculated from them, came out 2.02 to 2.05. Another prism of different dimensions gave values 2 to 2.1. The index of refraction (D line) of the resin was measured, and found to be 1.477 and  $1.477^2 = 2.18$ . The prisms were rectangular, and the sides of the base about 30 cm., 30 cm., 40 cm.

#### C. GRAWINKEL—INSULATION RESISTANCES OF UNDERGROUND CABLES.

(*Annales Télégraphique*, Vol. 18, p. 275.)

The underground trunk lines in the German Empire have, ever since they were

laid, been carefully tested for insulation every month. The insulation resistance (reduced to 15°) in all those referred to has always been found to be higher in summer, and this the author considers to be a fact, and not due to two sources of error to which he refers—viz., first, that the temperature corrections are made from English tables which were calculated for old-fashioned gutta-percha; secondly, that the temperature is determined by a copper resistance test which gives the mean temperature only of the line: e.g., if half the line were at 10°, and the other half at 20°, he shows that the insulation resistance will appear some 23 per cent. too high. With regard to the first, it may explain the fact that the resistance curve of the older cables is more approximately a straight line than those of more recent ones. Owing to the temperature difficulty, the author does not feel able to state definitely either that the cables have or have not altered slightly in insulation resistance.

### C. GRAWINKEL—ACCUMULATORS FOR TELEGRAPH WORK.

(*Elektrotechnische Zeitschrift*, Vol. 12, p. 555.)

This refers to the use of accumulators at the Berlin G.P.O. (cf. previous abstracts), the experiments in which began in 1885, and which are now working most satisfactorily. About 300 circuits are worked from one battery of 80 cells, and careful measurements show that for open-circuit working each line requires somewhat less than 24 milliampere-hours per day. For closed-circuit work about 300 milliampere-hours is required per line; but these circuits, being mostly low E.M.F. ones, it is found more convenient to keep separate and work off a few cells of their own. Even when working with Tudor lighting batteries (52 A.H.) there was a saving of 33 per cent. over Daniells, and this allowing 20 per cent. depreciation of accumulators, and the same supervision as for the copper cells, as the accumulators were of the ordinary lighting type, and were watched with the utmost and constant care. The form now adopted is a single positive between two negatives hanging from an insulating lid in a glass cell. The plates are 9 cm. × 15 cm. × 4 mm. thick ( $3\frac{1}{2} \times 6 \times \frac{5}{32}$  in.); the glasses 21 × 13 × 4.5 cm. broad; the plates are 11 mm. apart. The author again refers to the economy and other advantages of using such cells as these, kept constantly charging off a set of Daniells, for heavy work of all kinds and especially for cables; it is, of course, much more expensive than charging off the town lighting mains, but generally cheaper than putting down a dynamo and engine, or working off a large number of Daniells direct.

### E. MERCADIER—THE BI-TELEPHONE RECEIVER.

(*Comptes Rendus*, Vol. 112, p. 1416.)

This is the outcome of some former papers of the author (cf. previous abstracts), in which he showed that it was desirable to reduce the diameter of the diaphragm so that its fundamental should be higher than those in the ordinary human voice, and at the same time to reduce the thickness of the diaphragm. Working on these lines, he makes a pair of telephones which weigh only 50 grains (under 2 oz.), and are only 3 to 4 cm. diameter; these are connected by a loop of spring steel and furnished with soft rubber nozzles which

fit into the ears; they can then be worn for hours at a time without the least inconvenience when taking down dictation by telephone. The pattern has been passed by the Government Telephone Bureau as a receiver which is authorised to be used on their lines.

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### C. CARRÉ—THE PROPOSED ELECTRICAL UNDERGROUND RAILWAY FOR PARIS.

(*Lumière Electrique*, Vol. 42, No. 41, p. 72.)

The plan accepted by the Municipal Council is that of M. Berlier (cf. *Lum. El.*, vol. xxxiii., p. 276). The first section of the projected railway will start from the Bois de Boulogne to the Place de la Concorde, to be subsequently continued, parallel to the river, to the Vincennes terminus, thence to the Ceinture, taking the Lyons terminus on the way; thus roughly bisecting the city on its largest diameter.

On the first section there will be six stations, the generating one being at the outer end of the line. For the subsequent prolongation, there will be a generating station in the middle, near the Place de la Bastille. There will be a single tunnel, 5·8 metres diameter, with two lines of rails, the roof being about two metres below the level of the streets. The tunnel will be an iron tube built up as it progresses. The current will be picked up from a centre raised insulated iron rod 2 in. square, which will be connected to feeders run in a conduit between the two lines; the return is through the rails. It is intended to work at 400 volts, with two 25-H.P. motors on the leading carriage. According to the requirements of different times of day, different services of trains will be used: heavy service trains of five carriages, two minutes between trains and two minutes stops at stations; medium service, two carriages, two minutes interval and one minute stops; and light, two carriages, five minutes interval, one minute stops. The traffic is estimated at about seven million passengers per year, and it is also intended to use the line for goods traffic at night.

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### C. ZIEBENOWSKI—HIGH-SPEED INTER-URBAN RAILWAYS.

(*Paper before the Electrical Congress, Frankfurt. Electrotechnische Zeitschrift*, Vol. 12, Nos. 39, 40.)

The author proposes a line between Vienna and Buda-Pesth, a distance of 250 kilometres—say 150 miles—and a speed of 150 miles per hour, which he considers the maximum the wheels will stand without danger of flying to pieces. The trains would consist of one carriage, seating 40 passengers, carried on two bogies, with solid steel wheels 8 ft. diameter, the driving power being four 200-H.P. motors, working at 1,000 volts, which necessitates collecting, therefore, by contact from a centre raised rail, some 600 amperes. The up and down lines must be about 10 metres apart, to avoid the shock of air, when two carriages pass, blowing them off the rails. There must be no curves of less radius than 3,000 metres—say 1·8 miles—and for these the outer rail must be raised 15 cm. (6 inches). The power would be supplied from two stations, at 10,000 volts alternating, which would either be transformed down or redressed to direct current, at 1,000 volts.

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*Titles in heavy type denote that the Paper was read at a Meeting of the Institution.  
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